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Jan Mohr

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Vol. 2, No. 4, December, 1955 *Eugenics Quarterly*

EDITORIAL COMMENT

A.E.S. to Sponsor International Meetings

Arrangements have recently been completed for the American Eugenics Society to cooperate with other organizations in sponsoring two international scientific meetings. The International Congress of Human Genetics will be held in Copenhagen, Denmark, August 1 through August 6, 1956. The Tenth International Congress of Genetics will meet in Montreal, Canada, August 20 through August 27, 1958. Both of these meetings will be of outstanding interest and should have the full support of the membership of the A.E.S.

The International Congress of Genetics is among the oldest of the international scientific organizations. The present organization is the successor of the International Conference on Plant Breeding and Hybridization which was organized in London in 1889 and met in New York in 1902. The third session of this group was held in London in 1906, and the name was then changed to the International Congress of Genetics. Subsequent sessions of the Congress were held in Paris (1911), Berlin (1927), Ithaca, N. Y. (1932) Edinburgh, Scotland (1939), Stockholm (1948) and Bellagio, Italy (1953). The more recent sessions have included a section on human genetics, and many of the research reports presented to this section have been quite significant from the eugenic viewpoint. As advances in eugenic theory and practice must be based on a sound foundation of basic studies in genetics, the research communications presented to the Congress of Genetics will be of interest to all who are concerned with the biology of man.

The meeting in Montreal in 1958 will be the first session of this group on the North American continent in 26 years. The sessions will be held on the campus of McGill University. Professor J. W. Boyes is the chairman of the General Organizing Committee. This committee has been hard at work for many months making the necessary plans and arrangements. A generous grant from the Dominion Government, announced in September, has greatly facilitated the work of organization. Funds are being raised to provide travel grants to bring outstanding geneticists from other countries to Montreal. A number of professional societies and scientific organizations are joining forces to sponsor this meeting, with the Genetics Society of America playing a leading role. The officers of the American Eugenics Society are happy that this organization now has the privilege and the responsibility of sponsoring the Tenth International Congress of Genetics in association with other scientific groups.

The International Congress of Human Genetics to be held in Copenhagen next August will be the first of its kind. It may properly be considered to have had its origins in the International Symposium of Medical Genetics held in Rome during September, 1953, under the leadership of Professor Luigi Gedda. A group of outstanding medical geneticists felt that there was a real need for international meetings that would bridge the gap between laboratory genetics and clinical medicine. Medical genetics has become a specialty with close ties to both the animal geneticist and the physician. While the relations of medical geneticists to genetics as a basic science are adequately served by the International Con-

gress of Genetics, the relations to medical practice are not so well established. It was felt that this need might be met by an international meeting to which both medical geneticists and clinicians could be invited, and scheduled for a time and place that would avoid any conflict of interest with the International Congress of Genetics.

The invitation to hold the International Congress of Human Genetics in Copenhagen was issued by the Danish Organizing Committee with Professor Tage Kemp as chairman. National committees to sponsor this meeting have been formed in most of the countries of Western Europe and in some of the South American republics. The American Eugenics Society, the American Society of Human Genetics and the Genetics Society of America have joined hands to act as sponsors for this Congress in the United States and Canada. A committee has been formed to represent all these organizations under the chairmanship of Dr. Franz J. Kallmann of the New York State Psychiatric Institute and the College of Physicians and Surgeons of Columbia University. Professor Kallmann has been a member of the Board of Directors of the A.E.S. since 1949, is a charter member and past president of the A.S.H.G., and a member of the GSA since 1948. He has been a delegate to numerous international scientific meetings and is eminently qualified by his outstanding ability and experience to represent these societies at Copenhagen.

The final plans and programs for the Copenhagen meeting have not yet been announced. The provisional list of sections provides for papers and discussions concerning the applications of genetics to each of the medical specialties. There will also be sessions on genetic hygiene and genetic counseling, population genetics, differential fertility, assortative mating, intermarriage, twin studies and other problems of eugenic interest. (See pp. 253, 254.)

We hope that many members of this society will take advantage of the interesting opportunities offered and will attend these meetings. While we can hardly expect a large proportion of the membership to make the trip to Copenhagen, Montreal is not too distant. We hope that the A.E.S. will be well represented at both Congresses.

C. Nash Herndon, M. D.

LETTERS

Mr. Robert C. Cook
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Dear Bob:

You have made a constructive proposal in your article "Eugenic Hypothesis B" in the September 1955 issue of Eugenics Quarterly. The desire to bear sound, even perfect children is undoubtedly deep and universal. With proper education in human heredity, and with the inculcation of higher ideals of social and personal values, this "instinctive" feeling should be an important force furthering a eugenic distribution of births. Your able statement makes this very close and puts all of us in your debt.

It seems to me unfortunate that in the same article, you set up a straw man of a Eugenic Hypothesis which does not in fact exist, and proceed seriously to demolish it.

So far as I know, the first use of the words "Eugenic Hypothesis" occurred in *Preface* to *Eugenics* (Harper's, 1940). The phrase has since been amplified in various articles, and occasionally has been used by others, though I don't think it has ever come into common use. I may therefore be permitted again to define its meaning. The Eugenics Hypothesis states that in a society in which the social and psychological environment was properly developed from a eugenic point of view, the major proportion of children would be born to couples who were above the average in approaching the ideals of the society in which they lived. The trend in evolution would

then be determined by the ideals of each particular society, providing it is agreed, as you apparently agree, that on the whole there is a positive and not a negative relationship between achievement and the genetic endowment favorable to achievement. The hypothesis also postulates that those showing the best achievement will also be capable of giving their children a psychological environment better than the average, and prove themselves on the average the more responsible parents. Or to put it another way, that the more responsible parents in a eugenic form of social organization will also be the best citizens. There is a good deal of evidence for such a hypothesis. This does not require any genetic linkage between the different qualities which make for success. Nor does breeding to these traits require that the traits be linked.

At present, as you say in your article, there are many indications that we are breeding a major proportion of our children from those who are furthest from achieving the ideals for our society and also below the average in the psychological environment they provide for their children, Yet there are the beginnings of evidence that this need not be so. Among some groups of people, all of whom use family planning effectively, certain studies indicate that those who rise to the highest levels in business, education, or the professions and show other signs of achieving the ideals of our particular society, actually have more children than their less successful fellows. Such groups are the exceptions at present. The Eugenic Hypothesis holds that they can be made the rule.

The Eugenic Hypothesis does not rest on a quality you call philoprogenitiveness, or its genetic association with other qualities. I see the word philoprogenitiveness quite frequently in the British literature, but I have never used it and am not quite sure of its meaning. I have never supposed it applied to a genetic trait. Were the English women of 1860 who averaged over six children apiece during their married life endowed with a genetic philoprogentiveness lacking in their descendants of the 1930's who averaged little more than two children? Wasn't the change to smaller families due to a change in social and psychological pressures, just as was the change in family size among white-collar groups after the war?

The Eugenic Hypothesis recognizes the difficulty of making the social and psychological environment selective; that is, making it such that some people will have large families and others small families, or no children at all. But we have seen that in some groups even today just such a selection is taking place. We already have some idea as to the kind of social and psychological conditions which will be necessary to get eugenic results. Universal acceptance of family planning and a fully effective method of fertility control are the first necessities to a voluntary system, and we can conceive of no other. Your proposal in your Eugenic Hypothesis B is undoubtedly along the right lines, though it will be some time before there is much evidence for it.

Far from postulating a genetically based quality which you call philoprogenitiveness and using this to select for other qualities (which as you say are probably unrelated), the Eugenic Hypothesis presumes that man is chiefly affected by his environment in his decisions as to size of family, and that the environment can be manipulated to the desired end. It will take a lot of work to find out how to do this. It will help a lot if we can all agree on the hypothesis which we are using to set up our studies.

At least our Eugenic Hypothesis has this advantage. It is much easier to find agreement on the ideals of our society than on what we would consider the "ideal" man. Whatever the culture pattern to which we give allegiance, if we can make the Eugenic Hypothesis work out in practice, that particular culture pattern will be furthered.

With warm regards,

FREDERICK OSBORN

Comments on Mr. Osborn's Letter

My remarks on the "Eugenic Hypothesis" were based on a paper presented by Mr. Osborn at the annual meeting of the American Eugenics Society held in April, 1955.* In that paper, Mr. Osborn related eugenic progress to the enhancement of health in the population. As a selective criterion he thus defined health: "In recent years the word health has been given a broader meaning. It now includes almost all aspects of human well being. . . . Recently too we have learned that all aspects of health are interrelated. . . . Doctors are using the term psychosomatic, and I believe it can be applied to the family and the nation as well as to the individual. Health, like liberty, is indivisible. . . .

"So today we are using the term 'healthy family' in this broad and all inclusive sense." At the present time the parents ill-prepared for parenthood (and few of them "healthy" in this inclusive sense) are having the larger number of children. The cure for this is "to provide such conditions in our American society that fewer children will be born in unhealthy families, and that more than the average number of children will be born in healthy families."

That covers a lot of territory.

It is postulated that as reproduction becomes increasingly voluntary, the "healthier families" will tend to have the larger number of children. In my comment, I assumed that this was because a relationship was believed to exist between an inborn desire for children and the imposing catenation of qualities which Mr. Osborn bracketed in the designation "healthy." This relationship could only come to light when parenthood became voluntary: when the unhealthy, who assumedly did not want children, would not have them. This inborn desire for children related to the selective criteria set forth in the "Eugenic Hypothesis" was what I labeled "philoprogenetiveness." As I understood the argument, once parenthood had become voluntary this inherent (genetically based) desire for children would become the motivating force in positive selection.

Mr. Osborn's comment suggests that this assumption is not involved in the "Eugenic Hypothesis." Rather, "philoprogenetiveness" is to be built in where it is needed by environmental manipulation and suppressed where it is socially undesirable. It becomes in effect a highly labile characteristic, which is to be manipulated for what it is to be hoped will be its own future best interest by an enlightened society.

In the paper which I discussed in Philadelphia and which was the basis of my remarks in "Eugenic Hypothesis B," in addition to this phenotypically gargantuan quality called "healthiness," other qualities were mentioned in Mr. Osborn's discussion, which seemed to be correlated with a tendency, under conditions of free choice, to produce large progenies. Among these qualities are "responsibility," respectedness in the community," and "appreciation of values and happiness to be found in a larger family."

From a genetical point of view I find myself perplexed at this point. These very general and not very clearly defined qualities are not the unitary potentialities which genetic research has identified as being transmitted according to the Mendelian mechanism. In proportion as the population is labile and capable of being trained to "health" and to the other desired characteristics, then there is a question as to what extent characteristics to be manipulated have a genetic basis—or, being so manipulatable, to what extent their basis *is* genetic. If the responses to social manipulation are *phenocopies* rather than phenotypes having a genetic basis, then the result is not eugenics, rather it is euthenics.

If a eugenic program is to have any genetic meaning, it will only emerge when a system of genetic coordinates is related to such propositions as those put forth in the

*Published in *Eugenics Quarterly*, June 1955.

"Eugenic Hypothesis." It is a fact often attested by experience in plant and animal breeding that when phenotypic selection is practiced even for a few relatively simply inherited characteristics, progress in the enhancement or elimination of such characteristics has in practice proved to be disappointingly slow. If this is the case, then the question may be raised as to the extent to which a selection program based on such vague criteria is really going to be effective in reversing the counter-selective trends which the very recent pattern of mortality now obtaining in "advanced" nations appears to make inevitable.

The effectiveness of such a program could be subjected to a preliminary analysis on the basis of a series of postulates concerning the extent to which the characteristic "health" (or other desirable qualities) is displayed in the population and the differentials in fertility associated with the desired phenotype. In such a model several assumptions might be made regarding the heritability of the characteristics in question. This would permit an appraisal within the limits defined of the the influence on succeeding generations of various intensities of selection pressure.

In such an estimate, the fractions of the population affected by what we might call "released philoprogenetiveness" could be taken into account. In Mr. Osborn's commentary, he points out that the upper echelons of ability in business and the professions (and science?) have a higher reproductivity than their colleagues. This represents only a very small percentage of the population. The influence of these fertility differentials thus cannot be very great. With such an appraisal of the genotypic effects to be expected from such a program, it would be possible to decide whether the proposals were sound or should be modified, whether other motivations and other approaches were needed to give reasonable prospects of success.

It would also be possible to explore the extent to which the qualities defined as contributing to health might be genotypic. If lack of health should prove to be related to heterozygosis for morbid genes, then the "Eugenic Hypothesis" might have a more substantial genetic basis than I thought possible in my earlier critiques. Regarding this speculation there seems to exist very little information at the moment.

These points might be given a more nearly quantitative basis by such an analysis as is suggested above. With that as a basis, we might be able to substitute for hopes and opinions a more realistic appraisal.

ROBERT C. COOK

EUROPEAN HEREDITY REGISTERS

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In primitive societies, where external factors such as famine or epidemics may sometimes wipe out considerable parts of the population, it is reasonable to concentrate on counteracting these factors, and to pay relatively little attention to the diseases and defects due to hereditary causes.

The situation is different on a higher level of civilization, as in the North American and European countries. Since in these societies various external causes of disease have been fought successfully, while the internal causes have remained essentially unchanged, the latter have become more prominent. In such societies the health problem presented by hereditary influence on disease and maladaptation is of major importance. Several per cent of individuals suffer from some serious hereditary condition at birth or from a relatively early age.

The ever new and often increasing demands of society on the abilities of the individual is another point of interest in this connection. Reading is, for instance, today a required ability in most occupations in several populations where illiteracy was widespread even recently. And while dyslexia may be a serious handicap today, it could not manifest itself as a defect in an illiterate society. Like the spread of literacy in the case of dyslexia, other purely social changes may cause an apparent rise in the prominence and frequency of other defects influenced by heredity.

Even more than by such developments the interest in genetic causations has probably been increased by the recognition, also in wider circles, that the hereditary structure of human populations may not be considered unchangeable. The development of civilization has brought various new possibilities for genetic change. One of them is an increase in mutation frequencies due to a higher background radiation. Another, which has been discussed from earlier times, is that certain medical or social advances may eliminate selection against some hereditary diseases or defects and thereby lead to a gradual increase in their frequencies. Considering such possibilities, the need for intensified research in human genetics is strongly felt also from the point of view of practical medicine.

One of the possible means for augmenting our knowledge seems to be a wider use of heredity registers. The desirability of setting up such registers in countries where the conditions are favorable has been stresed at numerous occasions, for instance at one of the meetings during the recent World Population Conference in Rome (Böök, 1954).

The purpose of the present article is to suggest some of the ways in which heredity registers have been or may be useful; some of the possible sources of information are considered, to indicate in which circumstances it may be possible to achieve an efficient registration.

A heredity register is here taken to mean a permanent register of individuals suffering from serious diseases or abnormalities of possibly hereditary nature, including data on family relations and other information of significance in genetic investigations. The register is by definition presumed to be associated with an institution which has research in medical genetics as a permanent main task. Otherwise it could scarcely, during the long periods of time concerned, engage in research or practice of medical genetics with efficiency.

The idea of heredity registers has from time to time been considered in several European countries, and various attempts have been made to put it into effect. But lasting progress has been made only in a few places. In Denmark a heredity register was started in 1938 at the University Institute of Human Genetics, Copenhagen (T. Kemp). This register is so far the most comprehensive and will therefore be the main source of examples here. In Switzerland there is a register of more recent origin at the Medico-Genetic Center, Hôpital Cantonal, Genéve (A. Franceschetti). A first beginning has been made in a few other countries, for instance at Nederlands Instituut voor Praeventieve Geneeskunde, Leiden (P. J. Waardenburg). But in most of the European institutions for research in human genetics, the registration does not yet appear to go much beyond the simple keeping of data collected by the individual workers. In Norway an attempt is now being made to establish a heredity register at the Laboratory of Human Genetics, Oslo University, in cooperation with the Norwegian Health Services and with support from the Rockefeller Foundation.

In several countries more or less comprehensive special registers of various conditions which may be hereditary, such as blindness, deafness or psychosis, have been established at institutions with other chief interests than medical genetics. The institutions may be societies for the blind, hospital departments for eye diseases, mental hospitals, etc. Such registers will not be considered in the following discussion, which will be confined to heredity registers as defined above.

Possible Scope of Heredity Registers

The frequencies of some relevant diseases and defects will be discussed in order to suggest the numbers of cases to be registered. Only some larger groups will be mentioned, primarily mental deficiencies, psychoses, cases of blindness and deafness and malformations. Instead of aiming exclusively at the clearly hereditary cases, it is necessary to register some wider groups known to include a significant proportion of hereditary cases. It would be impossible for the reporting institutions or persons to select hereditary cases within such clinic-diagnostic groups for registration in any satisfactory way. Any such selection would be likely to make the material valueless for some of its most important purposes.

In the case of *mental deficiencies*, as for several of the other groups, the frequencies given for different countries or different times cannot easily be compared. This is partly because the criteria are apt to differ, being in practice social rather than biological. According to Böök (1953) the frequency of idiocy and imbecility taken together (I.Q. below 50) may conservatively be put at one-half per cent, and of the higher grades at two or three per cent.

The morbidity risk for *psychosis* is at least two per cent. According to Slater (1953) it may be estimated that 2.65 per cent of men and 2.93 per cent of women in England and Wales some time during their lives will be admitted to a mental hospital. On the Danish island Bornholm (about 40,000 inhabitants) the morbidity risk up to 55 years of age for all types of psychosis taken together is about 4.1 per cent according to Fremming and Strömgren (Kemp, 1951b), while the morbidity risk for schizophrenia and manic-depressive psychosis is about 0.9 per cent and 1.6 per cent, respectively. The frequency of psychotics in the general population at any chosen time is of course much lower.

The frequency of *blindness* varies considerably from country to country. In Norway there were in 1948, according to Holst (1952), at least 3,181 cases of blindness, that is, slightly less than one per thousand in the general population. About half these cases were considered to be of hereditary nature. Only about one-sixth were due to accidents or intoxicants.

Also the frequency of congenital or early *deafness* varies considerably from country to country. It may be 0.4-0.8 per thousand (Kemp, 1951b). Lindenov (1945) found that about half of 345 Danish cases examined in 1940 were of probable genetic origin. The rest were mainly due to various infections.

At least one per cent of the newborn have, according to Kemp (1951b), some serious *malformation*. But the frequency in the general population is lower because of the high mortality of these conditions. Among the most frequent malformations are harelip and cleft palate, spina bifida, congenital dislocation of the hip and various deformities of the extremities. A significant proportion of the malformations are hereditary or dependent upon a hereditary disposition. The same applies to several other groups of diseases or defects such as the following: various internal diseases (diabetes, bronchial asthma and scores of rarer conditions), diseases of the skin (for instance ichtyosis, xeroderma pigmentosum, albinism and epidermolysis bullosa), eye diseases (for instance cataract, aniridia and the retinitis pigmentosa group), nervous diseases (for instance epilepsy, neurofibromatosis and various abiotrophies) and certain other groups.

The special circumstances in the country or area concerned will be decisive for which conditions one should primarily include in the registration. The Danish register includes among other conditions mental deficiencies, psychoses, psychopathies, cases of blindness, deafness and language defects and various malformations. The total number of patients reported to this register, since it was started in 1938, now exceeds 200,000, and several thousands are added every year (Kemp, 1954); the number of inhabitants in Denmark is about 4.3 millions. The Swiss register aims at a similarly comprehensive registration, but the work is of a much more recent date and does not yet comprise a systematic registration of the groups covered by the Danish register. So far, about 700 reports a year are received, primarily on ophthalmological, neurological, dermatological, hematological and other special cases (Franceschetti, 1955). The Norwegian register, which has just been started, receives reports on mental deficiencies, cases of blindness, deafness and language defects and various other conditions.

Various Applications of Heredity Registers

A heredity register may primarily be of assistance in the following kinds of investigations or activities:

1. Estimation of the frequencies of the various hereditary conditions in the population and detection of possible changes in these frequencies with time.

2. Studies of the familial distributions or modes of inheritance of various diseases or defects and of their population genetics; selection, assortative mating, isolation, consanguinity. Studies of linkage relations may also be served by the register.

3. Estimation of mutation frequencies. An efficient and permanent registration of various conditions due to single genes would make it possible to observe any substantial increase in the mutation frequencies of these genes. This would be the most direct way to detect any genetic effect of an increased background radiation.

4. Genetic-hygienic counseling. The register may contain necessary information on the relatives of the patients, which may otherwise in many cases be difficult or impossible to obtain.

5. Studies of twins with diseases or defects influenced by heredity, to contribute to the evaluation of the relative importance of heredity and environment to the development of the conditions concerned.

6. Long-term planning of medical institutions and of the education of medical personnel for the diagnosis, treatment and rehabilitation of various conditions.

7. Early detection of cases which may

profit from early treatment, schooling or care.

Heredity registers have already been of great value in many of these ways. At the University Institute of Human Genetics, Copenhagen, *frequency studies* have for instance been carried out for chondrodystrophy, aniridia, hemophilia, surdomutitas (congenital or early deafness), epiloia, mongolism, osteogenesis imperfecta, harelip and cleft palate and club foot. Kemp (1950) has given a review of these and other frequency studies.

In the case of many of the conditions influenced by major genes, such as chondrodystrophy, phenylketonuria and hemophilia, the problem of definition is relatively simple. And in such cases it should not be unreasonably difficult for heredity registers gradually to achieve comparable estimates of the frequencies in different places and at different times.

The problem of definition is more difficult in the cases in which there is no sharp natural distinction between pathological and normal individuals. For some of these conditions, for instance psychosis, psychopathy and mental defect, the definitions used in practice are to a large extent social rather than biological, a significant factor being the inability of the individual to get along in society. The frequencies estimated on such grounds may of course be strongly influenced by trivial social or administrative factors, and the possibilities of demonstrating biological or genetic changes or differences may be correspondingly reduced or eliminated.

For some conditions, for instance mental defect, deafness and blindness, better definitions may be obtained by use of such tests or measurements which may permit a grading of individuals. Frequency studies may then be carried out for conditions defined as cases below a certain grade. If the cases included in a heredity register by partly social criteria are graded by such tests or measurements, the registration cannot be expected to be complete for the least severe degrees, but only for more serious cases. And accurate frequency studies based on the data of the register are of course only possible for those degrees for which there is sufficient reason to believe that the registration is complete. To make the data of the register more useful for frequency studies in the case of such conditions, the inclusion of cases should as far as possible be based directly on examinations, tests or measurements.

If a change in the frequency of a certain condition has been demonstrated, one is faced with the problem of whether or not it is due to a genetic change. For the single-gene conditions just mentioned, and other cases where environmental factors are known to be of little or no consequence, there is admittedly no such difficulty. But in the case of conditions like mental defect and psychosis, where environmental factors are known to be of significance, the question may be difficult to answer.

But observations of changes in frequencies from place to place and from time to time may of course be of very great interest even if an immediate genetic interpretation should be impossible. In some cases such observations might lead to an identification of relevant environmental factors, which in turn might facilitate the genetic interpretation. And the demonstration of a change of possibly genetic nature would be a strong stimulus to further research.

Heredity registers have also been of great assistance in studies of *familial distributions* and modes of inheritance of various diseases and defects; for instance the Danish register in studies of chondrodystrophy, harelip and cleft palate, hemophilia, surdomutitas, aniridia, myotonic dystrophy, osteogenesis imperfecta, epiloia and mongoloid idiocy.[1]

These investigations have generally been carried out by a physician with special

[1]Most of these studies have been published in the series, "Opera ex Domo Biol. Hered. Hum. Univ. Hafn.," Vol. 1-56.

training in the field concerned, who, starting with the data of the register and of other institutions that may possess information on the condition to be investigated, has traveled about the country to examine the patients personally and collect more accurate and comprehensive information on relatives.

Studies of this kind, to supplement and correct the data of the register, will undoubtedly be necessary also in the future even in the case of a register far better developed than any existing today. One may expect, however, that it will become possible in certain respects to base investigations more directly on the data of the register, as the reporting institutions gradually become more efficient.

The family studies will to an increasing extent come to include investigations of genetic linkage; so many chromosome markers are already available that data for the study of linkage should be collected as a routine procedure in most family studies. This often will make it necessary to travel around in the country to secure blood samples and other material from the patients and their relatives. A heredity register, containing information on all the relevant persons in the various districts, would permit this work to be planned and carried out efficiently without unnecessary travel.

Some of the investigations in which heredity registers have been of assistance have included estimations of *mutation frequencies.* This, for instance, applies to the Danish studies of chondrodystrophy, aniridia and hemophilia. Besides being of great theoretical interest, such estimates may be of a special practical importance in the atomic age, partly by giving a better foundation for theoretical estimations of the genetic sensitivity of man to irradiation, partly by giving a standard of comparison for later determinations of mutation frequencies during a possible rise in the background radiation.

Heredity registers may also be of value as sources of information in *genetic-hygienic* counseling. This work has achieved considerable scope in Denmark, partly because of the Danish legislation in this field, which permits abortion as well as sterilization on eugenical grounds. The University Institute of Human Genetics in Copenhagen has received more than 1,000 inquiries a year concerning genetic-hygienic counseling. In more than half of these cases the measure under consideration has been abortion. A considerable part of the inquiries have concerned cases of adoption. Cases concerning sterilization have been less numerous, and marriage and similar counseling problems have been relatively few. Most inquiries have come from the "mother-aid-institutions," the rest mainly from physicians in general practice and from hospitals or similar institutions. The pathological conditions concerned have been of great variety: malformations, cases of deafness, blindness or ear diseases, skin diseases, internal diseases, nervous diseases, mental deficiencies, psychoses and other conditions (Kemp, 1951a).

Sources of Information

The potential sources of information differ of course from country to country, and so do the practical possibilities of obtaining reports. The position of the public health services is of particular importance. Without their active support the establishment of an efficient heredity register would scarcely be possible. Most of the following possible sources should be available in all more civilized societies:

1. *Physicians.* School physicians, medical examination boards of the armed services, State district physicians, physicians in general or special practice.
2. *Hospitals, various special hospital departments and similar institutions.*
3. *Midwives and maternity hospitals.*
4. *Special schools or classes.* Schools for children with mental defect, blindness, deafness or defects of language.
5. *School psychologists.*
6. *Local committees for the care of chil-*

dren.

7. *Public or private institutions or societies concerned with economic or other support of certain groups of patients.*

8. *Special registers.* Registers of psychoses, mental defect, blindness, etc.

Before attempting to make use of any of these sources to establish a system of registration, it is of course advisable to decide exactly which groups of diseases and defects the heredity register should cover and formulate accurate and practical definitions of these groups. When all the sources necessary to detect all or practically all cases of a condition are utilized, it may be expected that the same case often will be reported by different sources. The information on an individual will be duplicated in part but often will be supplementary.

One may expect that even by using all available sources of information, the registration of various conditions may be rather inefficient in the beginning. However, as the reporting institutions and persons become better acquainted with the purpose and gain more experience, it should be possible gradually to attain a higher efficiency. Such an improvement should be easier to obtain if the institution with which the hereditary register is associated is engaged in the teaching of medical genetics. The Danish register as well as the recently established Norwegian heredity register are associated with University institutes which give regular courses in human genetics to medical students and others.

To achieve an efficient registration, it is of course important to make the reporting system simple. One of several points to consider in this connection is the coordination of the heredity register with possible special registers at other institutions. For instance, it would appear preferable (with certain exceptions) to have the reports, even on conditions covered by a special register, sent directly to the heredity register, and not first to the special register. This may simplify matters for the reporting institutions or persons: several of these would possess information on conditions which might be covered by different special registers, and it would clearly be simpler for them to report all cases to a single, central heredity register than to two or more special registers. Information desired by special registers may be conveyed as a routine procedure.

It may also be easier for the more comprehensive heredity register to maintain an efficient contact with the reporting institutions during the long periods of time concerned than for several smaller registers by separate efforts. And a direct contact between the heredity register, which has research and practice in medical genetics as a permanent main interest, and the reporting institutions offers the best prospects for a registration conducted with persistent regard to these tasks.

In Denmark one of the most important sources of information has been a number of special institutions for the care of patients with mental defect, epilepsy, blindness, deafness, language defects and invalidism. Shortly after the registration was started, the Ministry of Social Affairs sent an address to these institutions, stressing the importance for the special care system of the registration, and asking the institutions to report all cases coming under special care to the heredity register. All these institutions have responded to the request and have later sent regular and careful reports to the heredity register. These reports have been the nucleus of the Danish registration. The material has been supplemented from various other sources, such as psychiatric hospitals and institutions for psychopaths. Reports have also been received from other hospitals and physicians in general or special practice, often in connection with genetic-hygienic inquiries. Further, reports are received from the schools in Copenhagen, partly on children examined by the school psychologists, partly on children in special classes. Also valuable information is received from "social

committees," clergymen, teachers and others. (Kemp, 1945)

In the case of the recently established Norwegian heredity register, one of the first steps has been to secure reports on the children who are referred for examination or schooling to the special schools for cases of blindness, deafness, deficiencies of speech, reading or writing or of mental defect.

As regards the Swiss register, physicians in the various specialities have been a major source of information. All the existing associations (neurologists, internists, dermatologists, oto-rhino-laryngologists, etc.) have been approached and agreements worked out to obtain reports on hereditary conditions from the members of these associations (Franceschetti, 1955).

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THE USE OF PAPER CHROMATOGRAPHY IN HUMAN BIOLOGY*

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Methods

The technique of separating similar chemical compounds by means of their differential affinities for other materials was established in the earlier part of this century by the work of Tswett, who separated chlorophylls *a* and *b* from each other. The separation is based upon the fact that the rate at which molecules in a moving phase (usually liquid) pass over a stationary phase (usually solid) is related to the relative affinities of the molecules for the moving and stationary phases. Each chemical substance has its characteristic affinities for the two phases and hence possesses a characteristic rate of movement in the system. In practice a large vertical glass tube with an exit at the bottom is packed with some finely divided solid material such as starch or alumina, and the mixture of materials to be separated is added to the top of the column. A solvent, usually organic, is then passed slowly through the column and the different materials in the mixture become separated. Continued passing of solvent through the column (elution) will result in the emergence of the materials at the bottom, in an order inversely related to their affinities for the stationary phase. An alternate procedure, particularly useful for slowly moving molecules, is to stop the elution as soon as the materials are separated from one another in the column. The stationary phase is then carefully removed from the tube and divided into sections from which can be obtained the separated compounds.

*Studies on the heritability of biochemical differences which have been carried out at the Institute of Human Biology have been supported in part by McGregor Fund of Detroit and the U. S. Public Health Service, National Institutes of Health.

The original work was done with colored mixtures separated on columns of white powder. The process gave rise to descending bands of color; hence the name "chromatography." The method was soon extended to colorless compounds using other methods of detection: chemical, physical, and even biological for those substances recognizable only by their effects on living systems. A variety of materials also has been used for the stationary medium, a marked advance being the development of ion exchange resins which can be manufactured to satisfy a wide variety of needs.

During the early 1940's, a group of English investigators was experimenting with new combinations of mobile and stationary phases in the hope of discovering more efficient means for separating certain chemical compounds. Among the materials which they used for the stationary phase was cellulose. Cellulose is composed of many sugar units hooked together in long chains. Each of these sugar units has free hydroxyl groups which can attract water molecules and bind them loosely, so that cellulose fibers placed in a medium containing water will soon become covered with a thin film of water. Columns were therefore devised in which wet cellulose was the stationary phase and an organic solvent the moving phase. The cellulose was visualized as largely inert in the separation process, acting as a support for the stationary water.

This system gave satisfactory results, but a further idea occurred to the investigators: rather than pack the cellulose into a glass tube, why not use a piece of thick paper,

allowing the moving solvent to flow through the plane of the paper? This idea proved to be an excellent one and resulted in the publication in 1944 of a paper by R. Consden, A. H. Gordon, and A. J. P. Martin, on the principles of "paper" chromatography. For their studies of chromatography, including the contributions which led to the development of paper chromatography, Martin and R. L. M. Synge were awarded the Nobel Prize in Chemistry in 1952.

Characteristics

It will perhaps be advantageous at this point to enumerate some of the characteristics of paper chromatography, since these have been important in determining the uses which have been made of the technique. The earlier explanations of the mechanism of paper chromatography assumed the existence of two solvent phases — an immobile water layer attached to the surface of the cellulose fiber and a mobile organic layer filling in the remaining interstitial space. The differences in rate of migration which were observed for different compounds were thought to be a result of the differences in partition or relative solubility between these two phases, the more hydrophilic molecules spending a greater proportion of time on the average in the aqueous layer. Actually there seems little reason to suppose that two discrete phases exist. The creation of an interface from the initially homogeneous solvent mixture would require more energy than is likely to arise from the bonding of the water to the cellulose. All that is required, however, is a gradient in solvent composition from the slower moving solvent adjacent to the capillary walls to the faster moving solvent at the center of the capillaries. The likelihood of a molecule migrating away from a particular point within the capillary depends upon the chemical nature of the molecule and the solvent composition at that point. Thus molecules of different structure will be unequally distributed across the capillary and will therefore not show the same rate of migration through the capillary.

Regardless of the exact mechanism of the chromatographic separation in paper, it is essential that the compounds which are to be separated be present in quantities sufficiently small that their solubilities in the solvent not be exceeded. When this precaution is ignored, the moving solvent carries only so much of the material as it can dissolve, and the usual result is an ill-defined streak rather than a clear-cut separation. Since the volume of solvent which should contain all of a particular compound is exceedingly small — that contained in approximately one square centimeter of filter paper — the technique is successful only with very small quantities of material. With amino acids, for example, the best results are obtained when not more than about five-millionths of a gram of each amino acid are present in the mixture to be analyzed. Except in unusual cases, this has meant that paper chromatography, unlike the earlier column chromatography from which it was developed, is not useful for separating compounds in order to purify them for further manipulations. But it is useful in detecting the presence of compounds in unknown mixtures and in some cases in estimating the quantities of the compounds which are present.

In column chromatography, the usual procedure is to allow the solvent to percolate through the packed column until the compounds of interest have emerged at the bottom. This is desirable because further steps are usually to be carried out on the separated compounds. Since this is not true of paper chromatographic separations, there is little point in allowing the process to continue beyond the point of actual separation. Unless the molecular structures of the compounds in the mixture are very much alike, good separation can be obtained when the solvent has migrated only a short distance through the paper. For complex mixtures, a migration of twenty-five to fifty centimeters is common. For

easily separated substances, as little as five centimeters is sometimes sufficient. The filter paper is then removed from the solvent and allowed to dry. In the earlier studies, as mentioned above, the solvent was allowed to flow through the filter paper by gravity. A much more common procedure now is to place the filter paper vertically in the solvent with the mixture to be separated applied just above the level of the solvent. The solvent will rise through the paper until it reaches the top, at which point it will stop if the process has been carried out in a closed container in which the atmosphere is saturated with solvent. This "ascending" technique, first used by Williams and Kirby (1948), makes it unnecessary to pay close attention to the chromatogram during the resolution of the mixture.

Nothing has been said as yet concerning the detection of the compounds on the paper once they have been separated. If they happen to be highly colored, as was true of the chlorophylls originally studied, the problem of detection does not exist. Few of the compounds of interest have that gratifying property, however, and it is necessary to make them visible by other means. This can sometimes be achieved simply by viewing the resolved chromatogram under ultraviolet light, although this procedure is rather non-specific since so many compounds are to some extent fluorescent. Nevertheless it has proved useful in differentiating among a limited group of known compounds. The most common procedure is to add to the chromatogram a reagent which will produce a visible reaction with only a limited group of compounds. Because the amount of unknown material present on the chromatogram is extremely small, it is essential that the reaction be a very sensitive one.

The existence of a reagent which reacts with most amino acids and a few related structures has greatly simplified investigations of amino acid metabolism in the last few years. This reagent, called ninhydrin, will react to form a characteristic color in some cases with as little as half a microgram of the particular amino acid. Acid-base indicators have also been used to advantage, if the unknown substance departs sufficiently from neutrality. In fact almost any compound can be detected on a chromatogram by some reagent, although the specificity and sensitivity, and hence the utility, may vary widely with different reagents.

Another means of detection which has proved invaluable in studies involving radioactive materials consists in placing the chromatogram in contact with unexposed film. After a time appropriate to the intensity of the radioactivity, the film is developed and all parts of the chromatogram containing radioactive material appear as exposed spots at the corresponding point on the film. Such a technique is of particular use in ascertaining the fate of a labeled compound when added to an actively metabolising system. It has been largely through the use of radioactive tracers combined with paper chromatography that marked advances have been made in recent years in our knowledge of photosynthetic processes.

The rate of migration of a substance in a particular solvent system is expressed as an R_F value, defined as the distance moved by the substance divided by the distance moved by the solvent. Since this R_F value is a ratio, it is essentially constant, regardless of the distance moved by the solvent. It may be altered markedly, however, if the nature of the solvent is changed. This can be accomplished either by changing the proportion of water in the solvent mixture, or by changing the material used for the organic phase. The addition of small amounts of acidic or basic substances can also induce marked changes in R_F values, especially if the substance to be analyzed is itself somewhat acidic or basic. It is thus possible to prepare two-dimensional chromatograms in which an initial separation by one solvent is followed by another sepa-

ration by a second solvent in a direction perpendicular to the first. In complex mixtures, it is seldom possible to separate completely all of the compounds of interest with one solvent, since some of them will be so similar as to require an inordinate amount of time for the separation, with a gradual increase in diffuseness of the spots. By making use of the two-dimensional method, it is thus possible to separate with the second solvent many of the compounds which fail to separate clearly in the first solvent.

Uses

The versatility of paper chromatography has led to its use in virtually every area of chemistry and biology, including the study of man. A number of amino acids which had escaped detection by earlier methods have been found on chromatograms. One of the earliest applications of paper chromatography was made by C. E. Dent (1947) of the University College Hospital, London, who made a study of the increased amino acid excretion which is characteristic of Fanconi's syndrome. Amino acids have received by far the greatest attention in chromatographic studies, because of the availability of an excellent reagent for making them visible and because they occur in sufficiently high levels in blood, urine, saliva, etc., to make their study feasible. Sugars also received attention soon after the development of the chromatographic procedures, since a very sensitive color reagent was developed for them. However, they proved to be somewhat less interesting biochemically, both because so few of them occur at detectable levels in body fluids and because sugars have shown little individual variation which had not already been demonstrated by older methods.

One of the more useful applications of paper chromatography has been in the field of steroid metabolism. The other techniques available for separating and identifying such steroids as the sex and adrenal hormones are extremely laborious. The paper chromatographic procedures developed for steroids also have their limitations, it is true, because of the much greater solubility of these compounds in fat solvents as compared to water. Nevertheless, chromatography has provided a ready means of distinguishing between many of the steroid metabolites and of estimating roughly the quantities present. One technique which has been used in working with such fat soluble compounds is to "reverse" the phases of the filter paper. This is achieved by coating the cellulose fibers of the paper with a water repellent material, such as silicone, before the paper is placed in the solvent. The water-rich portion of the solvent then stays in the faster moving area of the capillary away from the capillary walls and the hydrophobic compounds stay closer to the capillary walls, resulting in a decreased rate of migration. Such substances as fatty acids and inorganic salts have also been studied in man by paper chromatography, but only to a limited extent.

The problem of quantifying the results of paper chromatograms has not been completely solved yet. Procedures have been developed for eluting the spots from the paper and applying some colorimetric method to the solution obtained. The laboriousness of these procedures largely nullifies the speed and simplicity of paper chromatography as compared to older methods. Another method has been to measure the area of the spot produced on the filter paper. This procedure is based on the fact that the greater the concentration of the compound to be determined, the greater the diffusion away from the center of the spot. This method has met with moderate success for some substances, but only if fairly large quantities of the material are present. The intensity of the color produced by reaction with a color-producing agent has also been used as a quantitative measure. Whatever the procedure applied, it is necessary to compare

the results with those obtained with known quantities of the same materials treated in exactly the same manner. On one-dimensional chromatograms, the standards and the unknown substance can be placed on the same piece of filter paper and carried through the same treatment. On two-dimensional chromatograms, it is necessary to have the standards and unknowns on different chromatograms, which introduces another source of error. By carefully standardizing the methods for preparing chromatograms, we have been able to reduce the error for some amino acids, as measured by color intensity on two-dimensional chromatograms, to about fifteen per cent. This applies to the more accurately measured amino acids only, since others may involve much greater error. For most of the amino acids this accuracy is sufficient for the detection of individual differences, since the variation between individuals is frequently of considerable magnitude.

The area of human variation which has been explored most thoroughly by paper chromatographic methods is that of amino acid excretion. These studies have dealt mostly with urine, but some have included blood levels of amino acids also. This interest in amino acids is largely a result of three facts: (1) the chromatographic procedures for amino acids can be made to yield quantitative information for most of the amino acids; (2) there are no alternative methods of amino acid assay which are uniformly superior to paper chromatography; and (3) there are known to exist rather large variations between individuals in amino acid excretion, some of which are known to be under genetic control.

The use of paper chromatography in studying Fanconi's syndrome has already been mentioned. In this case the results of amino acid analyses of the urine may help

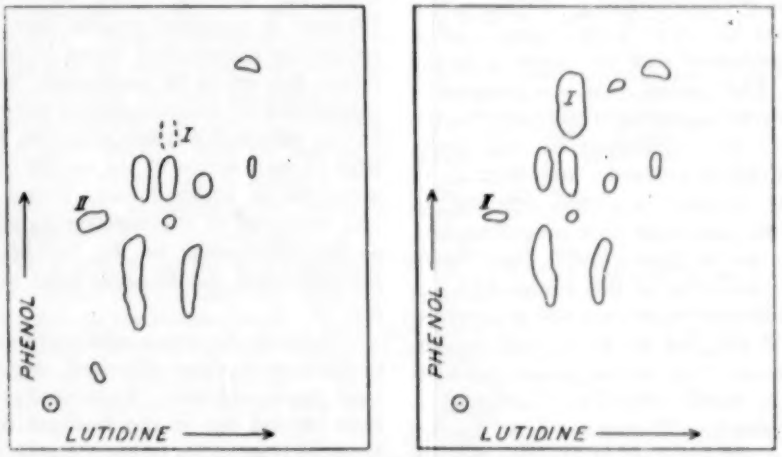

Figure 1. Diagrams of two two-dimensional amino acid chromatograms showing a difference in excretion rates for β-aminoisobutyric acid (I) and lysine (II). The area of the spot is an approximate measure of the amount of amino acid present. An aliquot of urine containing 50 micrograms of creatinine was applied to the lower left-hand corner of the filter paper. The two chromatograms represented were from a husband and wife and were collected the same morning. The chromatogram on the right is typical of a "high" excretor of β-aminoisobutyric acid while that on the left (showing little, if any, of Spot I) is typical of a "non-excretor."

establish a diagnosis. Another inherited disease which has as one aspect the excretion of large quantities of amino acids is Wilson's disease (hepatolenticular degeneration). The excretion of unusual quantities of cystine has been known to occur as a normal trait, although paper chromatography has revealed that the defect in renal tubular reabsorption actually affects lysine and arginine and perhaps a few other amino acids as well. The standard tests for cystine in the urine would fail to distinguish between these three conditions. Cystinuria is frequently first recognized by the formation of cystine stones. By chromatographic analysis of the urines of relatives of patients with cystine stones, it has been possible to detect affected individuals who may not yet have developed stones.

In addition to those amino acid anomalies which sometimes result in pathological conditions, several are known which apparently occur as normal variations. Perhaps the most striking of these is the condition in which large amounts of β-aminoisobutyric acid are excreted in the urine. This amino acid was completely unknown in nature until discovered in the urine of certain individuals by Dent (1948). With Crumpler, Harris, and Westall he reported the identity of this substance in 1951, and since that time several reports, notably that of Harris (1953), have shown that the excretion of this amino acid is a genetically controlled trait which occurs in about 10 per cent of the English population measured. It was suggested that high excretion results when an individual is homozygous for this recessive gene. Studies made in the Institute of Human Biology of the University of Michigan indicate that a racial difference exists in the frequency of this trait, since it was found that eleven out of seventeen Chinese excreted quantities in excess of any of twenty-nine Caucasoids (Sutton and Clark, 1955). Diet was shown to be relatively unimportant in the etiology of these differences, and the effects of urine dilution were largely removed by expressing the results as creatinine ratios. The metabolic significance of this amino acid is still unknown, as are the environmental influences which may alter its excretion. Another amino acid excreted in different amounts by the Chinese and Caucasoid populations is lysine. The excretion of this amino acid, also known to be influenced genetically, is much more complex, since twin and sibling studies reveal that individuals can fluctuate widely due to unidentified environmental factors (perhaps diet). Nevertheless, twin studies indicate a high heritability for the variation in lysine excretion and certain individuals are known to excrete consistently large quantities of lysine in environments which do not induce the same phenomenon in other persons. The higher excretion of lysine in the Chinese is interesting because lysine is one of the essential amino acids which are required in the diet since they cannot be synthesized in the body. Lysine is an important building block for proteins. Whether a marginal protein diet would reduce the excretion of lysine in high excretors has yet to be ascertained. Further studies seeking identification of the genetic factors influencing lysine excretion, knowledge of their action in the variety of environments in which human beings exist, and discovery of the possible significance of this interaction to the well-being of the individual should prove most interesting.

Differences in amino acid excretion less striking than those discussed above also have been observed. Twin studies have been carried out at the Institute for the Study of Human Variation, Columbia University, and at the Institute of Human Biology whose objectives included measuring the genetic component of the variability in amino acid excretion (Berry, *et al.*, 1955; Gartler, *et al.*, 1955). The results indicate that the excretion of several other amino acids may reflect genetic heterogeneity in the population, although the various genotypic differences are as yet unrecognized.

The idea that "patterns" of metabolism may exist within the normal range of variation has been particularly developed by Roger Williams at the University of Texas. Some of the studies of his group using predominantly paper chromatographic analyses of human fluids have already been summarized (Williams *et al.*, 1951). Chromatography would be a useful technique in the study of "patterns" of excretion, since a number of substances can frequently be measured simultaneously. Development of this work awaits objective means of describing and summarizing such patterns.

Contributions

The studies in amino acid excretion cited above have been presented to illustrate some of the possible applications of paper chromatography. Most of the studies which have been carried out by means of paper chromatography could also have been carried out by other techniques now available. The difference would have been in the time, expense, and technical personnel required. Paper chromatography is a simple procedure which can be carried out with a moderate amount of inexpensive equipment. Its low cost has made it available to many workers who do not have access to other techniques. It is still largely a research tool, although some hospitals are beginning to make limited use of it. It has already proved useful in recognizing certain biochemical differences between persons, and its usefulness should increase as refinements in technique are developed. It appears to be most promising as a tool for studies of human variability and may provide an efficient means of recognizing some of the innate differences which exist among individuals. Paper chromatography is, of course, only a chemical technique and its ultimate contributions will depend upon the ingenuity with which it is used.

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ECOLOGICAL AND SOCIAL FACTORS IN MORTALITY VARIATION*

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Introduction

THIS PAPER IS A PROGRESS REPORT of a study of mortality trends and differentials which has been underway more or less intermittently for the past 10 years. Although the study has been focused on North Carolina, some general analyses have been made of mortality trends and differentials in the south and in the nation. Within North Carolina, the study has been focused on an effort to explain variation in mortality rates between counties and subregions by means of multiple correlation analysis. The basic data for this study consists of state and national vital statistics and of United States Census data on population and housing.

Conventional mortality rates, deaths per 1,000 population, adjusted to the age distribution of the 1940 total population of the United States have been used in the study of differentials and trends by sex, color, and rural-urban residence. Deaths per 100,000 population, age-adjusted as above, were used in the analysis of causes of mortality. In the multiple correlation analysis, the dependent variable consisted of North Carolina 1950 county mortality rates adjusted by the indirect method to the age distribution of the total population of the United States in 1950. Independent variables consisted of about 14 different series of social and economic characteristics of counties, such as median income, manufacturing population, farm population, color, home ownership, fertility ratios, modern plumbing, educational level, hospitals, physicians, and subregional location.

The General Picture

The general trends and differences in mortality rates by sex and color and by residence and color in the United States, in the south, and in North Carolina are shown in Table 1. The rates in this table have been adjusted by the direct method to the United States total population of 1940.

Before attempting to make any summary statements about the mortality differentials in Table 1, I should like to raise some questions and to make a few comments about the use and misuse of certain statistical measures in describing and analyzing mortality rate differentials. It is a common but questionable practice to say that the mortality rate of one group is *X* per cent higher than that of another group. For example, a recent article on cancer included a number of such statements, one of which was to the effect that in a certain age group the mortality rate from respiratory cancer had increased 400 per cent within a twenty-year period. One might well ask: "Why not measure the change in terms of survival or vitality rates, that is, the complement of mortality rates?"

Suppose, for example, that we wish to determine from the data of Table 1 whether white people or nonwhite people had made the more progress in achieving good health. By using one measure it appears that white people made the most progress, whereas by using another measure it appears that nonwhite people made the more progress.

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Of the several measures that could be used, some tend to exaggerate the changes in the mortality rates of both groups; whereas, other measures tend to understate the magnitude of the changes. Consider for example the following statements about the changes in white and nonwhite mortality in North Carolina between 1940 and 1950.

1. The mortality rate of white people declined 24.8 per cent as compared with a decline of 19.1 per cent in the case of the nonwhite population.

2. The vitality rate of white people increased .252 per cent as compared with an increase of .284 per cent in the case of the nonwhite population.

3. The mortality rate of white people declined 2.5 points as compared with a decline of 2.9 points in the case of nonwhite population.

And to make a simple problem in human arithmetic even more confusing we shall add a fourth statement, based on life tables for white and nonwhite people in North Carolina:

4. Between 1940 and 1950 the average expected future lifetime of white people increased 5.2 years (8.1 per cent over 1940) as compared with an increase of 6.0 years (11.0 per cent over 1940).

Finally, we would like to suggest but not urge the use of a fifth measure based on the standard deviation of the dichotomal mortality-vitality variable, or, what is the same thing, the geometric mean of the mortality and vitality rates, *i.e.*, $\sqrt{m\,(1\text{-}m)}$. In terms of this measure, we can say:

5. Between 1940 and 1950 the mortality rate of North Carolina white people declined (and the vitality rate increased) 2.5 per cent of a standard deviation, as compared with a similar change of 2.4 per cent in the case of the nonwhite population.

Note, that in the case of statements 3 and 5, there is no inconsistency involved, attributable to the confusion between vitality and mortality rates, as is found in statements 1 and 2.

Those who wish simple and meaningful answers to this little statistical puzzle will no doubt select statement 3. For quick comparisons the simple point difference between two mortality rates has a lot to be said for it. For one thing, the difference between two mortality rates has a lower limit of 0 and upper limit of 1 (or of 1,000, if such a base is used). In this respect the difference between two mortality rates is a measure of degree of association, much like Pearsonian coefficient of correlation. As a matter of fact, the difference between two mortality rates is mathematically speaking a regression coefficient, *i.e.*, the regression of mortality on whatever dichotomous variable is represented by the two groups being compared e.g., color, sex, rural-urban residence. For example, we might ask "what is the degree of association between mortality and sex in the United States?" Table 1 shows the answer to this question to be .0031, that is, .0100—.0069. For the relationship to have been perfect, that is +1, all males in the United States alive at the beginning of 1950 must have died during the year and no females could have died.

Now, for a few comments on the data of Table 1.

First. Mortality rates in all groups are relatively low and are falling rapidly. This is considered to be a most desirable situation. However, the trend does have at least two important social consequences. (1) The decline in the mortality rate, accompanied by continued high birth rates, has brought about a rapid increase in our total population, the ultimate consequences of which need not be discussed now. (2) We have made it possible for a large percentage of our population to live to very old ages. There is real danger that the length of life will be extended faster than the arts of living and institutional arrangements can be developed to make old age living both a productive and pleasurable experience. As of 1950, 25 per cent of North Carolina white females at birth had an expected future lifetime of 85 years or

TABLE 1
AGE-ADJUSTED MORTALITY RATES BY SEX, COLOR AND RESIDENCE
1940 AND 1950, UNITED STATES, THE SOUTH, AND NORTH CAROLINA*

Residence, Color, and sex	United States 1940	United States 1950	The South[b] 1940	The South[b] 1950	North Carolina 1940	North Carolina 1950
All Groups	10.7	8.4	11.6	8.7	11.4	8.7
White	10.2	8.0	10.2	7.8	10.1	7.6
Nonwhite	16.2	12.2	16.1	12.6	15.2	12.3
Male	12.1	10.0	13.0	10.3	12.8	10.2
Female	9.3	6.9	10.1	7.1	10.0	7.2
Urban	11.4	8.9	13.8	9.7	13.1	9.8
Rural	9.8	7.4	10.2	7.9	10.6	8.1
White Urban	10.8	8.5	12.0	8.5	11.0	8.2
White Rural	9.3	7.1	10.2	7.9	9.7	7.3
Nonwhite Urban	18.1	13.1	20.0	14.3	18.2	14.0
Nonwhite Rural	14.4	10.9	14.0	11.0	13.6	11.2
White Male	11.5	9.6	11.7	9.5	11.6	9.3
White Female	8.8	6.4	8.6	6.1	8.6	5.9
Nonwhite Male	17.5	13.5	17.4	13.6	16.4	13.5
Nonwhite Female	14.9	10.9	14.8	11.0	14.0	11.1

*Adjusted to the total United States population of 1940.
[b]"The South" in this table includes all 17 of the states classified as southern by the United States Census Bureau.

more as compared with 80 years for North Carolina white men. The corresponding expectancies for nonwhite women and men was found to be 80 and 75 years, respectively. The median years of future life expected for the North Carolina sex and color groups in 1950 was found to be as follows:

White male	71.2 years
White female	77.3 years
Nonwhite male	62.0 years
Nonwhite female	65.7 years

The mean life expectancies were found to be from 2.5 to 4.5 years less than the medians.

Second. Of the population classes represented in Table 1, the mortality differentials between white and nonwhite people are greatest; differences by sex are next in order, and differences by rural-urban residence are third. As between the south, North Carolina, and the United States, mortality differentials are very small. Within color, sex, and residence groups the differences are even smaller—small enough to be ignored so far as this analysis of averages is concerned. (See Figure 1.)

Third. Between 1940 and 1950, throwing all groups together, age-adjusted mortality rates in North Carolina and in the south as a whole declined more than the corresponding rates in the nation. However, some color and residence differences are noted. In the decline in mortality rates from 1940 to 1950, southern whites fared better than the whites of other states; but southern nonwhites fared worse than the nonwhites of other states. Similarly, mortality of southern urban people (both white and nonwhite) declined more than the mortality of corresponding groups in other states; but the rates among southern rural people declined less than among rural people in other states.

Fourth. In general, in all regions, between 1940 and 1950 mortality differences between whites and nonwhite and between rural and urban groups were getting smaller; but differences by sex in the white population were widening. Mortality differentials between males and females of the nonwhite population remained constant.

Mortality Differences by Cause

In this study to date we have only been able to analyze mortality differentials by sex and color, in North Carolina, in the south and in the nation for eight major causes for which the basic data were readily available in the Vital Statistics reports for the United States. Table 2 and Figure 2 show the results in compact form. The causes are arranged in order of mortality

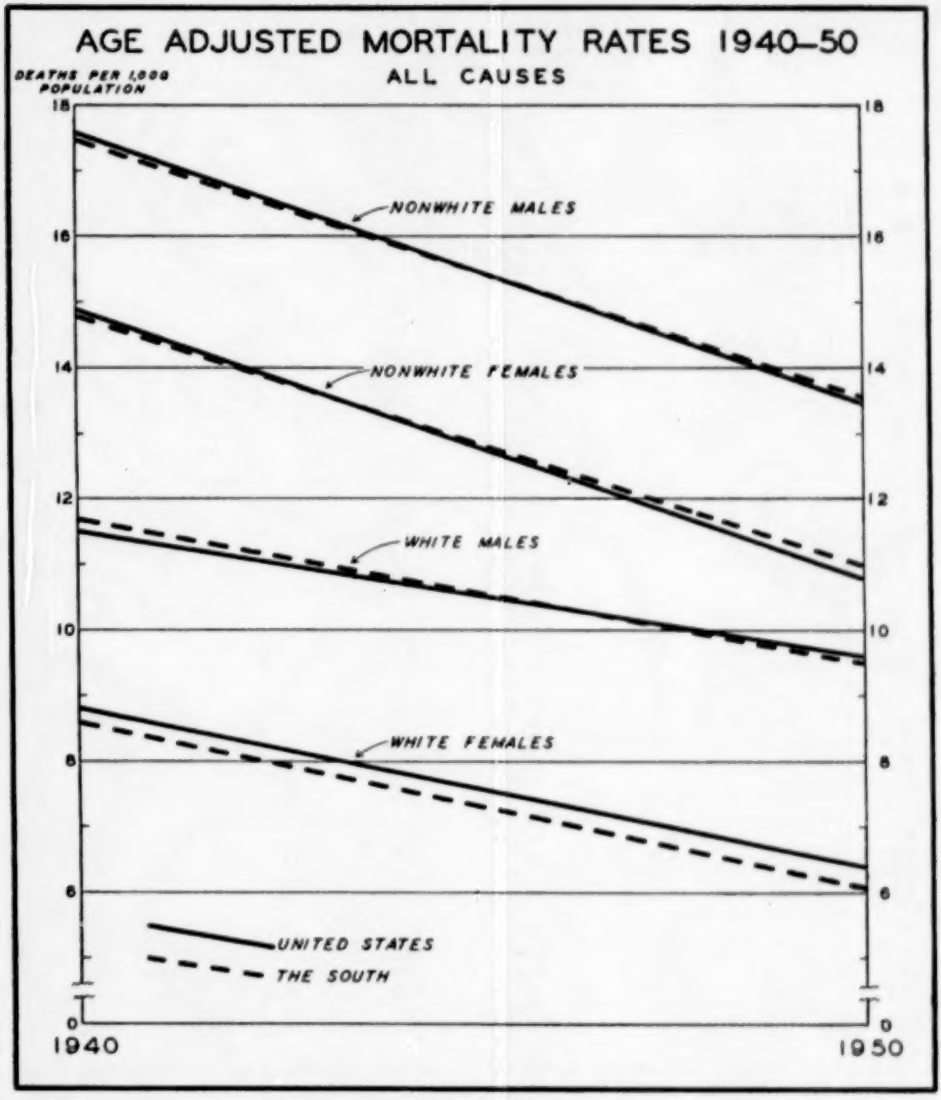

FIGURE 1

rates for the United States in 1950 and all rates have been adjusted to the age distribution of the 1940 United States total population. However, the rates are expressed in terms of deaths per 100,000 population. About four-fifths of all deaths in the United States in 1950 were found in the eight causes represented in Table 2, and between 1940 and 1950 the percentage of deaths in these eight causes has increased in all sex and color groups and in all regions.

Time and space does not permit a detailed analysis of the data in Table 2 but a few general statements can be made.

TABLE 2
DEATHS PER 100,000 POPULATION
UNITED STATES, THE SOUTH, AND NORTH CAROLINA
1940 AND 1950*

	United States		The South		North Carolina	
	1940	1950	1940	1950	1940	1950
All Causes	1076.4	836.2	1156.1	866.5	1138.7	869.8
White Male	1148.9	960.4	1170.4	946.7	1160.8	930.8
White Female	877.5	641.1	862.1	610.3	862.0	590.9
Nonwhite Male	1747.7	1346.1	1736.7	1356.8	1644.9	1348.7
Nonwhite Female	1492.2	1082.6	1484.6	1100.7	1399.9	1111.1
Diseases of the Heart and the Circulatory System						
White male	344.3	384.0	285.4	352.6	293.7	378.3
White Female	234.2	224.8	176.9	193.0	187.4	198.9
Nonwhite Male	349.3	408.8	318.2	407.0	289.0	446.0
Nonwhite Female	306.6	344.4	271.2	336.6	242.1	378.7
Cancer and Other Malignant Tumors						
White Male	118.0	131.1	86.5	113.0	73.6	91.0
White Female	98.0	119.5	98.7	108.1	92.6	97.0
Nonwhite Male	83.2	126.1	61.9	110.6	61.4	86.7
Nonwhite Female	119.2	132.2	103.7	122.0	104.1	119.4
Intracranial Lesions of Vascular Origin						
White Male	87.3	87.6	101.9	93.7	128.5	116.6
White Female	82.7	80.1	84.8	77.9	103.1	92.3
Nonwhite Male	139.8	144.5	160.2	164.0	192.4	186.8
Nonwhite Female	160.5	154.1	173.8	183.4	182.7	180.9
Accidental Deaths						
White Male	100.7	81.2	102.7	91.6	100.7	86.3
White Female	43.2	30.7	44.2	32.6	41.3	28.2
Nonwhite Male	121.4	107.7	120.5	111.0	111.5	110.7
Nonwhite Female	44.2	38.7	43.8	38.2	41.7	34.3
Pneumonia and Influenza						
White Male	70.2	27.2	79.8	30.6	71.0	26.8
White Female	55.6	19.0	90.6	24.8	78.8	25.0
Nonwhite Male	152.5	63.4	156.3	63.7	125.3	60.9
Nonwhite Female	117.8	50.7	129.8	54.8	110.6	55.1
Tuberculosis (All Forms)						
White Male	44.0	23.3	49.9	26.3	38.8	15.3
White Female	27.9	10.1	39.3	14.8	23.6	8.0
Nonwhite Male	148.1	82.0	114.2	67.2	142.1	56.7
Nonwhite Female	117.0	53.2	101.0	48.1	109.6	47.2
Nephritis (All Forms)						
White Male	81.8	16.4	105.6	20.0	137.3	22.2
White Female	67.6	12.5	75.7	14.7	105.2	14.9
Nonwhite Male	174.8	44.7	194.0	51.9	222.7	50.9
Nonwhite Female	156.1	41.5	162.2	46.7	196.6	49.2
Diabetes Mellitus						
White Male	20.6	11.3	16.2	10.0	20.3	10.2
White Female	32.9	16.4	19.5	12.1	20.3	10.9
Nonwhite Male	14.9	11.8	11.8	11.1	12.8	15.6
Nonwhite Female	32.1	22.8	25.5	22.0	25.3	18.7

*Adjusted by the direct method to the age distribution of the 1940 total population of the United States.

First. Among the eight causes, in North Carolina, in the south and in the nation as a whole, mortality from the following causes has increased in most sex and color groups: diseases of the heart and the circulatory system, and cancer and other malignant tumors. Practically no change occurred in deaths due to intracranial lesions of vascular origin. Decreases occurred in the remaining five causes but were most pronounced in nephritis, pneumonia and influenza, and tuberculosis.

Second. In diseases of the heart and the circulatory system, mortality rates were highest among nonwhite males, white males being second highest, nonwhite females fall in the third position, and white females having the lowest rates of the four sex and color groups. Although the southern states had mortality rates from heart disease slightly lower than those of other states in 1950, the differential gap appeared to be closing. That is to say, the south had a greater increase in heart disease mortality

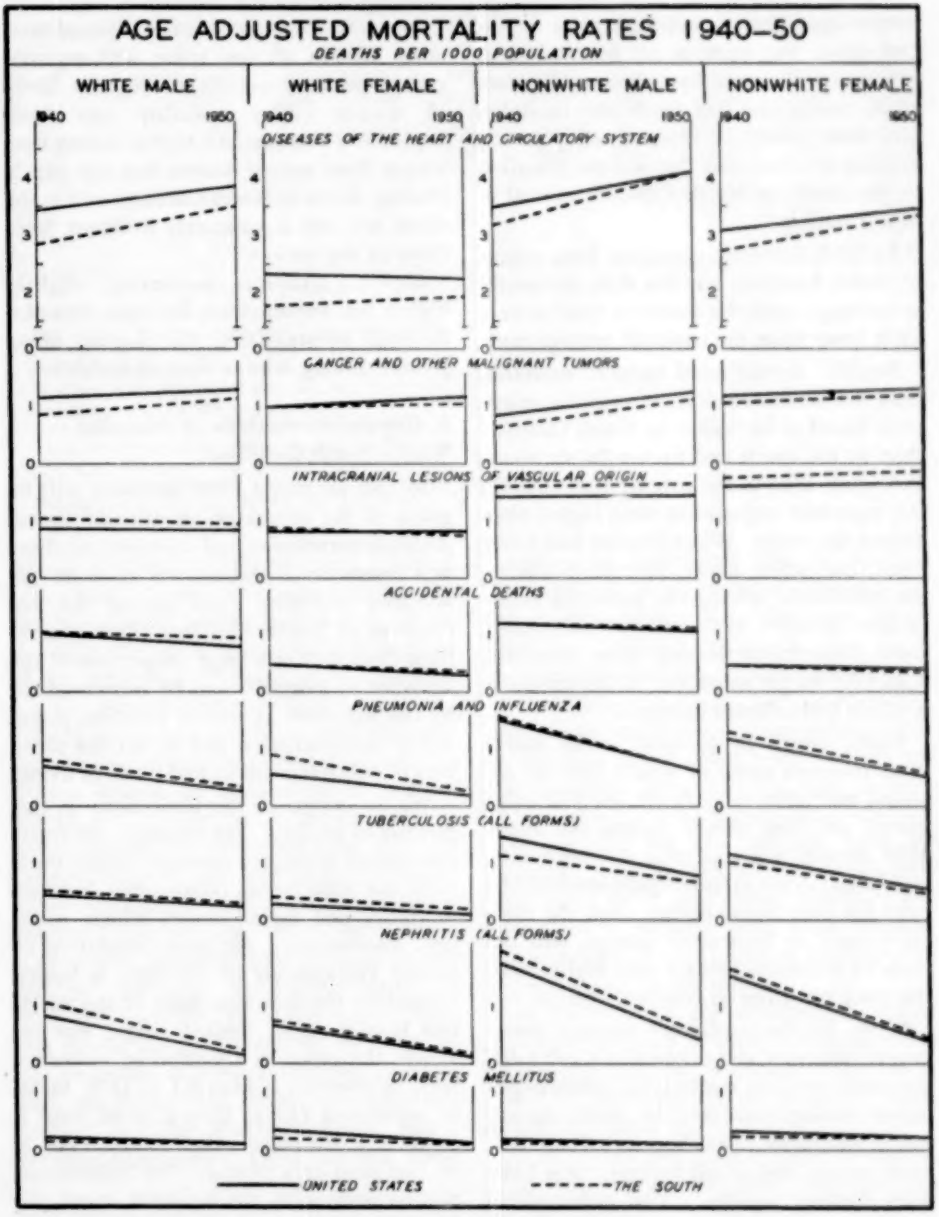
AGE ADJUSTED MORTALITY RATES 1940–50
DEATHS PER 1000 POPULATION
WHITE MALE
WHITE FEMALE
NONWHITE MALE
NONWHITE FEMALE
1940
1950
DISEASES OF THE HEART AND CIRCULATORY SYSTEM
CANCER AND OTHER MALIGNANT TUMORS
INTRACRANIAL LESIONS OF VASCULAR ORIGIN
ACCIDENTAL DEATHS
PNEUMONIA AND INFLUENZA
TUBERCULOSIS (ALL FORMS)
NEPHRITIS (ALL FORMS)
DIABETES MELLITUS
UNITED STATES
THE SOUTH

FIGURE 2

than did other states.

Third. There are no great differences in cancer age-adjusted mortality rates by sex and color, but rates in all sex and color groups in all areas have increased. Nonwhite males, who had the lowest mortality rate from cancer in 1940, experienced the greatest increase, and this was the situation in the south, in North Carolina as well as in the nation.

In 1950, however, mortality from cancer in North Carolina was less than the southern average, and the southern rates were a little lower than the national average rates.

Fourth. Age-adjusted rates of mortality from intracranial lesions of vascular origin were found to be higher in North Carolina than in the south and higher in the south as a whole than in the nation. Rates among the nonwhite population were higher than among the white. White females had lower rates than white males, but there was no sex differential among the nonwhite population. Between 1940 and 1950, mortality from intracranial lesions have decreased somewhat in the south but in the nation as a whole little change occurred.

Fifth. Death by accident is the fourth most frequent cause of death, but the adjusted mortality rates in all sex and color groups declined slowly during the 1940–1950 decade. Sex and color differentials in accidential death rates are quite marked; the rates for men being higher than the rates for women in both color groups, and the rates for nonwhite being a little higher than the rates for white in both sex groups.

Sixth. In the respiratory diseases, pneumonia, influenza, and tuberculosis, color differentials are most marked, the white population having considerably lower age-adjusted mortality rates; but in all sex and color groups and in all regions, rates have been falling rapidly. The tuberculosis mortality rate in North Carolina is lower than the southern average rate. However, southern whites have about the same age-adjusted death rate from tuberculosis as the national average for whites; while southern nonwhites have slightly lower rates than the national average for nonwhites.

Seventh. The nephritis age-adjusted mortality rate in all sex, color, and regional groups declined rapidly during the 1940–50 decade. The mortality rate from nephritis is substantially higher among nonwhites than among whites but the gap is closing. Rates in North Carolina and in the south are not significantly different from those in the nation.

Eighth. Diabetes mortality, slightly higher for women than for men, has also declined substantially, the decline being greater among whites than nonwhites.

A Correlation Analysis of Mortality Within North Carolina

In this section, a brief summary will be given of the results of an attempt to use multiple correlation and regression analyses as a means for explaining variation in age-adjusted mortality rates among the 100 counties of North Carolina. Our general hypothesis is that a large proportion of the variance in mortality can be accounted for by the variation in county location, population characteristics, and by certain measures of cultural patterns and levels of living.

We are aware of the limitations in this method of analysis. For example, the inter-correlation of county averages cannot show fully the patterns of relationship between mortality and social factors within counties. Furthermore, the inter-correlation of county averages on all variables is handicapped by the fact that most of the variables have a rather limited range. For example, the range in age-adjusted mortality rates by counties is from 5.1 to 13.7. Ideally, we should like to have a range from 0 to 1000, a situation which would exist if we had mass data available for individuals. Similar statements can be made about the limited range of most of the independent variables.

In spite of its weaknesses, the correlation method does have some usefulness. Logically and mathematically, it can be demon-

TABLE 3
CORRELATION MATRIX
RELATION OF COUNTY AGE-ADJUSTED MORTALITY RATES TO TEN VARIABLES
NORTH CAROLINA 1950

	X_2	X_3	X_4	X_5	X_6	X_7	X_8	X_9	X_{10}
X_2	−.5036	.1463	.0505	−.4467	−.5473	.2228	−.2651	−.8159	−.7098	−.0861
X_3		−.3781	.1595	.2520	.2219	.0576	.5026	.3953	.2163	−.2838
X_4			−.6855	−.1480	−.0411	−.6771	−.0517	−.2718	−.2535	.7571
X_5				−.0194	−.2226	.5612	−.1055	−.0904	.0179	−.6887
X_6					.7192	.0500	.2677	.6279	.5468	.0089
X_7						−.0618	.2758	.7052	.6110	.0552
X_8							−.4144	−.0266	−.0991	−.6690
X_9								.3058	.3475	−.0771
X_{10}									.8397	−.0327
X_{11}										−.0663

X	*Variable*
X_2	Per cent population living on farms
X_3	Per cent of workers employed in manufacturing
X_4	Per cent of population classed as nonwhite
X_5	Per cent of families owning homes
X_6	Hospital beds per 10,000 population
X_7	Physicians per 100,000 population
X_8	Location of county in Mountain area × 100
X_9	Location of county in Piedmont area × 100
X_{10}	Per cent of homes with modern plumbing, etc.
X_{11}	Median grades of formal schooling completed by adults × 10
X_1	Deaths per 10,000 population, age-adjusted

strated that except for somewhat unusual conditions, linear interrelationships between variables within groups will be reflected in the correlations between averages of the variables on a group basis. The basis for the grouping of individuals is crucial. Ideally the groups should be small in population and the placing of individuals in the groups should be a randomized process. Or to put the matter differently, ideally there should be no correlation between county location and the variables under study. Obviously counties are not groups of randomly selected individuals and there is a great deal of correlation between county location, mortality, and various social and economic variables. However, these difficulties can be partially overcome by introducing measures of county location as variables, and by carrying on the correlation procedures within relatively small homogeneous geographical areas. In this study, so far we have experimented with two methods of dealing with county location: (1) correlations were computed within the four major geographical areas of the state —Mountain, Piedmont, Coastal Plain, and Tidewater and (2) two of the four major regions were introduced into the correlation analysis as dichotomous variables—that is, every county of the state was given a score of 0 or 1 for each area. Any given country, for example, could be classified as to its location as being "in" or "not in" each of the four regions. A mountain county, for example, is given a score of "1" on the mountain area variable and a score "0" on each of the remaining area variables. We did not try the method used by Margaret Hagood—that of introducing east and west, north and south distances between each county and lines passing through some central or other conveniently located point in the state.

In addition to the selection of major geographical areas as variables, our general hypothesis and the availability of census data by counties led us to select eight other variables:

2. Per cent population living on farms.
3. Per cent of workers employed in manufacturing.
4. Per cent of population classed as nonwhite.
5. Per cent of families who owned their homes.

6. Hospital beds per 10,000 population.
7. Physicians per 100,000 population.
b. Location of county in Mountain area.
c. Location of county in Piedmont area.
8. Per cent of houses equipped with running hot water and modern bathroom facilities and which also were not dilapidated.
9. Median grades of formal schooling completed by adults.

The dependent variable in this analysis was the age-adjusted general mortality rate by counties. Each county rate was adjusted by the indirect method to the age distribution of the 1950 total population of the United States. In 1940 we found that age-adjusted rates computed for North Carolina counties by the "indirect" method were highly correlated with rates computed by the "direct" method; the coefficient of correlation being .95. For 1950, therefore, we used the "indirect" method because it requires less clerical labor.

Although the dependent variable has been broken into four sex and color series, we have not correlated each of these four series with the eight independent variables. Our assumption has been that the advantages of such a procedure would not justify the additional labor involved, particularly since sex and color could be controlled by multiple and partial correlation.

We did find some use for the adjustment of county mortality rates by sex and color. Correlation between the male and female rates and also between the white and nonwhite rates provide rough tests of reliability. For example, the coefficient of correlation between male and female rates (white only) was found to be .645, a highly significant correlation; and between white and nonwhite rates, the coefficient was found to be .452, also highly significant. We could hardly expect the white-nonwhite correlation to be as high as the male-female (white) correlation because the two sex groups live together and are subjected to the same family, cultural, social, and economic influences; whereas, the two color groups are certainly not influenced by the same family factors, and only to a limited degree by the same social and economic factors.

Another rough reliability test is found in the correlation of the 1940 and 1950 age-adjusted county general mortality rates, the coefficient being .715, a highly significant value. Here again, we had no ground to suppose that the correlation would be much higher. Errors due to reporting, chance errors, unequal changes in social and economic factors and in medical services by counties would all tend to lower the correlation between the 1940 and 1950 mortality rates.

The unweighted county age-adjusted rate was found to be 9.9, just 0.1 below the weighted mean; and the standard deviation was found to be only 1.69. The range from lowest to highest rate was 5.1 to 13.8, a difference of 8.7, or approximately 5 standard deviations. We call attention to the fact that the standard deviation is relatively low, which means that there is considerable uniformity in mortality rates among the counties. In other words, we have only a small amount of variance in mortality to account for by means of multiple correlation.

Of the eight variables examined only the following were found to make a statistically significant contribution toward explaining the variation of mortality from county to county.

Per cent of families who owned their homes.
Per cent of population classed as white.
Residence in the Mountain area.
Residence in the Piedmont area.
Ratio of hospital beds to population.

Table 4 shows the partial regression coefficients and their standard errors. Other results not shown in Table 4 are:

1. R^2, the multiple coefficient of determination = .7420
2. R^2, the multiple coefficient of determination corrected = .7130
3. Holding area location of county con-

TABLE 4.
RESULTS OF MULTIPLE CORRELATION ANALYSIS OF COUNTY AGE-ADJUSTED MORTALITY RATES
NORTH CAROLINA 1950

Variable Number**	Mean $\bar{X}_i$	Standard Deviation s_i	S.P.R.C. $B_{oi}\ldots$	P.R.C. $b_{oi}\ldots$	Standard Errors of P.R.C.
2	43.46	19.64	—.1034	—.0889	.1133
3	23.50	14.24	—.0429	—.0509	.1108
4	26.70	18.30	.2282	.2106	.1029*
5	56.55	12.15	—.3130	—.4351	.1230*
6	25.70	19.65	.2235	.1922	.0718*
7	52.92	28.76	—.1343	—.0788	.0551
b	25.00	43.30	—.4647	—.1813	.0449*
c	34.00	47.37	—.3033	—.1081	.0334*
8	26.83	11.93	.0290	.0411	.2206
9	76.14	6.88	—.0720	—.1768	.2908
0	99.05	16.89	——	——	——

**For full identification of variables see Table 3.
*Sig. at .95 level.

stant, $R^2_{0(23456789).bc} = .3564$

4. Reducing the analyses to only four highly significant variables,

$$R^2_{0.45bc} = .7126$$
$$R^2_{0(45).bc} = .2831$$
$$R^2_{0(bc).45} = .2385$$
$$r^2_{04.bc5} = .04261$$
$$r^2_{05.bc4} = .14591$$

Two important conclusions can be stated at this point:

1. There is apparently a significant difference between the age-adjusted mortality rates of home owners and nonowners; and this difference is greater than the difference between the age-adjusted mortality rates of white and nonwhite people.

2. Even after eliminating the influence of home ownership and color, there is a significant difference in age-adjusted mortality rates by geographical area. Mountain people apparently have the lowest mortality rates in the state and Piedmont people the next lowest. Even after eliminating the influence of tenancy and color in eastern North Carolina, age-adjusted mortality rates are still high in that area. As compared with the remainder of the state, the partial regression coefficients indicate that the Mountain area, other variables being held constant, have 1.8 fewer deaths per 1000 population, and that the Piedmont area have 1.1 fewer deaths per 1000 population.

Correlation Analysis Within Four Major Geographical Areas

Another method of eliminating the effect of regional location of county is that of making a correlation analysis within regions. This has been done by means of a short-cut formula, the effect of which is to convert the measures being intercorrelated to deviations from regional means.[1] This approach does not yield sets of correlation and regression coefficients for each region but rather a set of average coefficients, an average of the coefficients within the four regions. So far in this study we have made a separate analysis for only the coastal plain region of the state.

The correlation matrix based on the within-region analysis is shown in Table 5. Table 6 shows the means, standard deviations, and the partial regression coefficients of the within-region analysis.

A comparison of Tables 3 and 5 and Tables 4 and 6 shows that some coefficients based on the within analysis are about the same as those based on the state-wide analysis, whereas certain other coefficients are considerably different in value, some higher, some lower, and some even being of the

[1] $\Sigma x^2 = \Sigma X^2 - \Sigma X_a \cdot X_a - \ldots - \Sigma X_i \cdot X_i$
$\Sigma xy = \Sigma XY - \Sigma X_a \cdot Y_a - \ldots - \Sigma X_i \cdot Y_i$
in which a and i denote state regions.

opposite sign. Interestingly enough, however, the two approaches show the same independent variables to be significantly correlated with mortality. See Table 7. Also the two approaches yield similar results with regard to the influence of the regional location of county.

For example, compare

$$R^2_{0(45),be} = .2831$$

with the corresponding within region coefficient

$$_iR^2_{0.45} = .2672$$

Of the two approaches, the statewide analysis, in which regional locations of counties are introduced as independent variable, has some advantages. For one thing, it permits an evaluation of the separate influence of each region.

Summary

1. Mortality rates in all sex, color, and residence categories are falling rapidly.
2. Of the population classes represented in Table 1 of this paper the mortality differences between white and nonwhite people are greatest; differences by sex are next in order, and differences by

TABLE 5.
CORRELATION MATRIX
RELATION OF COUNTY AGE-ADJUSTED MORTALITY RATES TO EIGHT SOCIO-ECONOMIC VARIABLES*
WITHIN THE FOUR MAJOR GEOGRAPHIC AREAS OF NORTH CAROLINA, 1950

	X_3	X_4	X_5	X_6	X_7	X_8	X_9	X_0
X_2	—.5393	.3137	.1354	—.4630	—.6023	—.8382	—.6714	—.0961
X_3		—.2480	.0705	.0858	.0903	.2720	.0313	—.0328
X_4			.4453	.0005	—.0137	—.2627	—.2753	.3577
X_5				—.1461	—.2734	—.2842	—.1328	—.4934
X_6					.7051	.5910	.5209	.2826
X_7						.7042	.6341	.1718
X_8							.8260	.1605
X_9								.0616

*For identification of variables, see Table 3.

TABLE 6.
RESULTS OF MULTIPLE CORRELATION ANALYSIS OF COUNTY AGE-ADJUSTED MORTALITY RATES
WITHIN THE FOUR MAJOR GEOGRAPHIC AREAS OF NORTH CAROLINA, 1950*

Variable Number	Area Means				Standard Deviations	S.P.R.C.'s	P.R.C.'s
	Mountain	Piedmont	Coastal Plain	Tidewater	σ_i	$B_{oi\ldots}$	$b_{oi\ldots}$
2	51.04	36.20	52.70	34.83	17.90	—.1630	—.0946
3	24.92	33.47	13.91	14.94	11.58	—.0476	—.0427
4	5.24	25.38	45.52	34.94	11.20	.1808	.1677[1]
5	68.36	54.76	43.48	60.22	7.75	—.4129	—.5535[1]
6	27.40	33.03	18.78	18.33	18.61	.3626	.2024[1]
7	49.84	63.97	51.13	38.61	27.31	—.2182	—.0830
8	26.38	31.91	21.09	25.33	11.18	—.0300	—.0279
9	74.96	79.47	71.83	77.00	6.23	—.0772	—.1287
0	79.48	97.23	115.96	108.06	10.39	——	——

*For identification of variables see Table 3.
[1]Only the b_{oi}'s of variables 4, 5, and 6 were found to be significant at the .05 level.

TABLE 7.
PARTIAL REGRESSION COEFFICIENTS BY TWO METHODS OF ANALYSIS

Variable	Partial Regression Coefficients	
	Statewide analysis: regional location constant	Within region analysis
Per cent population nonwhite	.2106	.1677
Per cent families owning homes	—.4351	—.5535
Hospital beds per 10,000 population	.1922	.2024

rural-urban residence are third.

3. In general, between 1940 and 1950 mortality differences between white and nonwhite and between rural and urban categories were decreasing, but differences by sex within the white population were increasing. Mortality differences between the sexes of the nonwhite population remained relatively constant.
4. Between 1940 and 1950, throwing all categories together, age-adjusted mortality rates in North Carolina and in the south as a whole declined more than the corresponding rates in the nation.
5. Among the eight major causes analyzed, in North Carolina, in the south and in the nation as a whole, age-adjusted mortality rates from the following causes have increases in most sex and color categories: diseases of the heart and circulatory system, and cancer and other malignant tumors. Practically no change occurred in mortality due to intracranial lesions of vascular origin. Decreases occurred in the remaining five causes. No analysis has yet been made by cause and residence. Other differentials and trends by sex, race, region and cause may be read directly from Table 2.
6. A multiple correlation analysis of North Carolina age-adjusted mortality rates by counties reveals no significant association between rural-farm residence and age-adjusted mortality rates.
7. Home tenure in North Carolina, the correlation analysis shows, is more highly correlated with age-adjusted mortality rates than is color.
8. Even after holding color and home tenure constant, a significant correlation between county location and age-adjusted mortality rates was found. Mountain counties were found to have the lowest rates; Piedmont counties the second lowest; while Coastal Plain counties had the highest rates.
9. With regard to comparing mortality rates of one population category with another, this paper suggests that simple point differences are more meaningful and realistic than are the ratios between two rates. A point difference between the mortality rates of two population groups is mathematically identical with the regression coefficient of mortality on the population categories (considered as a dichotomous variable) and has, like a correlation coefficient, limits of 0 and 1.

WOMEN'S OPINION ABOUT SIZE OF FAMILY IN THE NETHERLANDS

Attempts to Measure Desired Size of Family*

SJOERD GROENMAN
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in The Netherlands
Amsterdam

FERTILITY IS MEASURED TODAY chiefly by using factual data on births. Various time-series and analytical breakdowns of the numbers of births can be obtained if the necessary statistical materials are available. Statistics on births may be correlated with age of mother, social status, church affiliation, duration of marriage, etc. The meaning of such statistics is twofold. They may be interpreted in terms of an actual situation within an area; they may serve also for estimates of the future population by means of extrapolation.

If we try to interpret demographic materials, trends may be discovered and motivations investigated simply by differentiating among the data and isolating and correlating the material. It must be taken for granted, however, that conclusions have to be formulated very carefully. In addition to these purely statistical methods, direct gathering of information from those who are in the reproductive age group can be of great value.

Scope of Institute's Research Project

The Institute for Social Research in The Netherlands, located in Amsterdam, carried out a research project in 1950 and 1951 in which the direct method of inquiry was used apart from analysis of vital statistics. A mail questionnaire was sent to women residing in municipalities in The Netherlands with more than 100,000 inhabitants who were planning to marry (for the first time) and who were under 50 years of age. In The Netherlands a couple that wants to get married must give notice of its planned marriage some weeks before. The municipalities where the brides are residing publish names, dates of marriage, etc. Each week the addresses of the prospective brides were sent to the Institute for Social Research, and the mail questionnaires were then distributed immediately.

The questionnaire opens with a short explanation of the purpose of the inquiry. Then eight questions follow with space for comments. The questions relate to the ages of the woman and her prospective husband, occupation, church affiliation, and the number of children of the woman's parents. The two most important questions, however, are these:

1. If it rested with you, how many children would you consider ideal for your future family?
2. Do you hope to have children in the shortest possible time?

The Committee for the Study of the Birth Rate at the Institute of Social Research has had these aims in mind:

1. To obtain information as to the attitude of the population in matters of family size that would be useful in clarifying the official demographic data—
2. To gather materials of importance for making estimates of future population trends—

In carrying out the first aim, the spontaneous remarks of those answering the questionnaires were of paramount importance. As regards the second aim, attitudes concerning family size were investigated to discover views held just before

*Presented at the World Population Conference, Rome, 1954.

marriage. There is reason to hope that the information gained in this way will be of great value for the interpretation of known trends in the birth rate and that extrapolation of these trends will become somewhat easier. Certainly the expressed views are only just that—views—whether they will be realized or not remains to be seen. Even if these views are not realized, they may suggest possibilities. They may indicate whether or not observed trends, for instance, the process of leveling the fertility differentials, will maintain themselves. It should be stressed here that two follow-ups should be organized in order to make the research project more fruitful and to secure comparable data. First, the same investigation can be repeated within a couple of years. Second, another project investigating women married during a five-year period may be carried out after some time.

Another point should be emphasized. Conventional birth rate statistics represent the fertility of a great many marriage cohorts. The birth rate results from births deriving from marriages of different durations. In the project described here and in the proposed follow-up the latest cohorts are involved.

Validity of Data

The Committee for the Study of the Birth Rate considered whether or not it would be possible to obtain information in the way described above. The whole question of attitudes concerning family size and eventual family planning is very complex, of course. Everybody should not be expected to give information about these matters whatever the research method. The mail questionnaire was anonymous, but counteracting this advantage was the drawback that non-response in a mail questionnaire is always large.

Another consideration is that there would certainly be a great bias in the data. The sample naturally could not be representative, and, therefore, the Committee did not strive at such a sample. The same number of forms was distributed in each municipality regardless of the number of inhabitants. Emphasis was laid upon comparisons among the answers in different categories. For the total of breakdowns along lines of age, occupation, church affiliation, etc., the publications of the committee for the Study of the Birth Rate may be consulted. In this paper only some of the data will be discussed.

A total of 11,000 forms were sent to women who had given notice of their intended marriages. In each municipality 1,000 forms were distributed. The percentages of returns varied from 14 to 26, and usable answers from 12 to 24. The average of total returns was 20.6 per cent, and of usable returns, 18.5 per cent. In the large cities—Amsterdam, Rotterdam, and The Hague—the total of 1,000 distributed forms was reached very soon. Hence these returns (728) were handled separately. The data of the other eight municipalities (1,526) were analyzed later on.

In The Netherlands religious views are very important in social life and especially in family matters. Therefore it is important that the usable returns be numerous enough to make comparisons among persons with different church affiliations. Such comparisons are given in Tables 1 and 2.

The total number of returns shown in Tables 1 and 2 are satisfactory. The Roman Catholics by far made the greatest contribution, though they are under-represented.

The breakdown by occupation of the future husband is shown in Table 3.

The over-representation of non-manual workers is conspicuous even without the presence in Table 3 of comparative data representing the composition of the total labor force. The effect of this bias was far greater than the disproportional representation of the religious groups.

Certainly a nationwide representative sample would have produced other results than the present investigation. The comparisons among the various categories, how-

TABLE 1

USABLE RETURNS FROM WOMEN BY CHURCH AFFILIATION

	Amsterdam Rotterdam The Hague	Other 8 municipalities	Total
Roman Catholic	142	592	734
Dutch Protestant	227	359	586
Calvinist (Reformed)	51	76	127
No church affiliation	212	248	460
Others	39	75	114
Total	671	1350	2021

TABLE 2

USABLE RETURNS FROM COUPLES BY CHURCH AFFILIATION

	Amsterdam Rotterdam The Hague	Other 8 municipalities	Total
Roman Catholic (both)	106	549	655
Dutch Protestant (both)	169	265	434
Calvinist (both)	46	62	108
No church affiliation (both)	168	200	368
Other combinations	182	274	456
Total	671	1350	2021

ever, have validity. If we consider the data on family size reported in the 1947 census and distinguished as regards church affiliation and occupation and compare these data with our collected material, they are to a great extent similar. The same leveling trend is to be observed, and this time we have not only figures on family size but also stated wishes and comments that are often very striking.

TABLE 3

OCCUPATIONS OF FUTURE HUSBANDS

	Amsterdam Rotterdam The Hague	Other towns
Commercial middle class	29	93
Professional and higher intellectuals	73	105
Other non-manual workers	287	630
Manual workers	277	523
Total	666	1351

Meaning of Results

The purpose of this summary is only to describe this project which was undertaken in The Netherlands to measure the desired size of family. Therefore no elaborate survey of the results will be given; only some conclusions will be mentioned.

In addition to the difference in opinion between Roman Catholics and Calvinists (who want to a much greater extent a family of three or more children) and the others, it is significant that among female manual workers there are proportionately more women who wish a small family than among non-manual workers. The same difference exists between women marrying manual workers and those whose future husbands belong to the professional and other intellectual classes. Most women hope to have fewer children than their parents had. The minority, which hopes to have more children, came from families with one or two children.

As several women desired an indefinite number of children, no exact average family size was computed. Table 4 gives information as to the number of children the women in different occupational groups would like to have.

Table 5 shows how the size of the family of the women's parents is related

TABLE 4

% of Women Desiring Less than 3 Children, by Occupation or Probable Future Occupation of Husband and Area of Residence

	Amsterdam Rotterdam The Hague	Haarlem Groningen Arnhem Enschede	Utrecht	Tilburg Nijmegen Eindhoven
Commercial middle class	69%	50%	40%	12%
Professionals and higher intellectuals	31%	33%	17%	9%
Other non-manual workers	47%	43%	34%	19%
Manual workers	69%	60%	49%	28%
Average for area	55%	47%	38%	22%

TABLE 5

Size of Family Desired by Women With Smaller and Larger Sibships, by Area of Residence

	Size of family of women's parents					
	Haarlem, Groningen Arnhem, Enschede		Utrecht		Tilburg, Nijmegen, Eindhoven	
	fewer than 3	3 and more	fewer than 3	3 and more	fewer than 3	3 and more
Women desire:						
fewer children than parents	4%	77%	2%	74%	4%	74%
as many as parents	38%	17%	20%	14%	21%	13%
more than parents	58%	6%	78%	12%	75%	13%
	100%	100%	100%	100%	100%	100%

TABLE 6.

(A) % of Women Desiring Family of Less Than 3 Compared to (B) % of Women with Sibship Less Than 3, by Church Affiliation and Area of Residence

	Amsterdam Rotterdam The Hague		Haarlem Groningen Arnhem Enschede		Utrecht		Tilburg Nijmegen Eindhoven	
	A	B	A	B	A	B	A	B
Roman Catholic	38	17	29	14	22	10	17	10
Dutch Protestant	58	36	49	33	43	31	46	34
Calvinist	23	23	19	10	14	14	—	—
No church affiliation	72	36	66	45	75	29	69	15
Others	54	36	45	25	41	24	57	14
Average by areas	55%	32%	47%	31%	38%	22%	22%	13%

to the size of the family they would like to have themselves.

Table 6 demonstrates the decrease in desired family size, differentiated by church affiliation.

The most conspicuous finding is that the Calvinists ("Gereformeerden") are less in favor of a small family than the Roman Catholics.

On the whole the results of the investigation were very satisfactory in that they clarified the motivations behind registered birth rate trends. The mail-questionnaire method proved workable. Analysis of the spontaneous comments led to interesting results. The women who wished less than three children mentioned overpopulation, financial circumstances, the possibilities of education and professional training, and

the housing shortage. In addition, several women mentioned the large size of their parents' family as a warning example. Confidence in the providence of God as to the number of children was expressed chiefly by Roman Catholic and Calvinist women.

A NEW STUDY OF AMERICAN FERTILITY

Social and Psychological Factors

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Background

During the past few years, a committee[1] of social scientists under the guidance of the Milbank Memorial Fund has been working toward the development of a new study of social and psychological factors affecting fertility. The interest of this committee is an outgrowth of the now virtually completed Indianapolis Study[2] which has been the only major inquiry in this country into the question of why people have the number of children they do. The Indianapolis Study provided a fund of valuable information on fertility. It also represented a radical innovation in the kinds of research undertaken by demographers in attempting to go beyond the collection of descriptive population data in order to uncover the underlying social and motivational processes.

The results of the Indianapolis Study, important as they are, are nevertheless representative of conditions existing in the United States during the depression decade. Much of the interest in that study arose from a concern about the possibility that the falling birth rate might result in a declining population. Since then, of course, the demographic situation has changed markedly. The last 15 years has been a period of unparalleled increase in the national birth rate. This new situation has provoked a great deal of speculation about whether this change reflects a real increase in size of completed family.

The answer to the questions of what has happened and what is likely to happen to the birth rate in the near future will be obtained eventually from official statistics supplemented by the national sample survey of fertility expectations and desires currently being conducted by Ronald Freedman and P. K. Whelpton.[3] However, our knowledge and understanding of the basic social and psychological processes that are associated with these demographic changes will remain inadequate unless these processes themselves become the specific objects

[1] A Steering Committee and its Technical Staff have carried on the work of the Study thus far. Aside from the authors of this article, its members are Frank W. Notestein (Chairman); Ronald Freedman; Philip M. Hauser; Frank Lorimer; Donald Marquis; Frederick Osborn; Lowell J. Reed; and P. K. Whelpton.

[2] The results of the Indianapolis Study are published in a series of four volumes with a fifth and final volume to be published in the near future. *See* P. K. Whelpton and Clyde V. Kiser (Editors), *Social and Psychological Factors Affecting Fertility* (Milbank Memorial Fund, New York). The four volumes were published in 1946, 1950, 1952, and 1954, respectively.

[3] *See* P. K. Whelpton, "A Study of the 'Expected' Completed Fertility of a National Sample of White Women," in *Current Research in Human Fertility* (Milbank Memorial Fund, New York, 1955), pp. 106-112.

of investigation. In the proposed research, primary emphasis is placed on the isolation of some of the causal connections between fertility behavior and certain cultural values, patterns of family life, personality characteristics, and a number of other social and psychological variables. Thus, the new study emphasizes "depth" rather than "range" and concentrates on basic research rather than on problems of forecasting.

The Study Design

To achieve our research objectives most efficiently, we have felt it necessary to restrict the sample to couples all of whom have recently had births of the same order and to follow these couples over a period of time with repeated interviews. This decision was governed by two specific considerations. First, the fact that size of completed family is the result of a complex sequence of events suggested that each stage of this sequence warranted separate study and analysis. There is some evidence for this in the Indianapolis Study. Secondly, since the Indianapolis Study was conducted *after* the couples had largely completed their families, there has been a recurrent question about whether the relationships observed were the cause or the effect, or both cause and effect, of differences in fertility. The present study expects to overcome these difficulties by contacting a sample of couples soon after the birth of their second child and following these couples over a period of four to five years in an attempt to isolate and define the factors related to the occurrence and spacing of a third child. The interval between the second and third child has been selected for study since it represents a significant link in the changing size of completed family and is the crucial point in the difference between an ultimately increasing or decreasing population.

More precisely, the study plan calls for drawing a probability sample, stratified at least for regional composition, of 1500 native-white women who currently reside in the 271 counties defined by the Census Bureau as in the Standard Metropolitan Areas. This comprises about 56 per cent of the total United States population. The primary criterion of the sample will be that these women have had a second child approximately six months prior to the first interview. Additional eligibility requirements that are being imposed in order to assure sufficient homogeneity for analysis are to exclude all couples who have been married more than once, wives older than 35 and husbands older than 45 years of age. Other restrictions on pregnancy histories are being imposed that will eliminate couples with histories of stillbirths, plural births, and losses of children through death. The main differences in this sample design from that of the Indianapolis Study are that couples of all religious persuasions will be included, that it will cover a more comprehensive population, and that it will be restricted to women with the same number of children.

The question naturally arises as to how such a specifically defined sample can be most economically procured. Two general sources have been considered: the State Vital Statistics Offices and hospital records. The Vital Statistics Offices clearly constitute the more economical alternative. A survey of all 53 offices, conducted about a year ago, indicated that it would be mechanically feasible to draw a sample satisfying most of the criteria described previously. Our current estimates are that between 75 and 80 per cent of the final sample would be correctly classified, in terms of our eligibility criteria, on the first run. Such factors as histories of previous marriages, not recorded on the birth cards, and any errors that might have entered into the registration, coding and punching operations of the birth records, will be the main sources of loss. Oversampling is thus indicated, not only for this type of loss, but also to compensate for some unknown, but

probably small, proportion of couples who cannot be traced because of changes of address during the six-month interval.

No attempt has yet been made to secure the cooperation of the Vital Statistics Offices, so we cannot say at this time what the final source of the sample will be.

The Fertility Variable and the Logic of Analysis

The study design includes a series of three interviews which will be conducted exclusively with wives, although a short questionnaire on two subjects specifically relevant for men will be left with the husbands. The first interview will be the longest (about an hour and a half) and will be devoted to obtaining several classes of data. Following a few general "ice-breaking" questions, a number of screening questions will be asked to verify the couple's eligibility for inclusion in the sample. Then data on the dependent variable of fertility will be obtained. This will include fairly elaborate information on pregnancy and contraceptive histories, as well as desires and expectations for a third child, and general attitudes toward practices of family limitation. In the latter connection, we are hoping to derive a scale (meeting the requirements of the Guttman scaling procedure) on a variable we have defined as the Psychological Availability of Contraception. This is intended to be a measure of the level of moral and aesthetic acceptability of the idea and techniques of family limitation. The conceptual organization of the study regards the occurrence and timing of a third birth to be a direct function of a couple's contraceptive behavior, which in turn is dependent on both the strength of a couple's desire to have or to postpone another pregnancy, and the extent to which some mode of contraception is psychologically available to the couple. Thus, the primary analytical task following the first interview will be to account for the variation of these two variables as well as the variation in previous contraceptive practice and spacing of births.

The function of the second and third interviews (to be spaced approximately 18 to 24 months apart) will be severalfold. First, we will be interested in testing the accuracy of our predictions of third births from our information on past contraceptive practice, stated fertility desires and expectations, and the wide array of social and psychological data collected in the first interview. Secondly, we plan to collect up-to-date records on income, financial perspectives, jobs, and other factors that may have changed in the interim. The ultimate function of the re-interview structure will be to improve and reformulate our hypotheses, a task that in many instances will be facilitated by more probing interviews with those couples who do not conform to expectations. The main rationale for a third interview, aside from these general objectives, is to allow a sufficient amount of time to elapse from the second child for most of the third pregnancies to occur. It is estimated that a total of around 73 per cent of fecund couples who have had two pregnancies eventually will have three or more pregnancies, that around 10 per cent of these women will already be pregnant again six months after the birth of their second child, and over 85 per cent will have become pregnant within the 4 to 5 year period covered by the Study.

The Independent Variables

Thus far, little has been said about the kinds of factors that we consider relevant for an analysis of fertility. To reiterate: this Study is visualized primarily as basic research into the fundamental social and psychological factors relevant to fertility. In deciding which specific variables to incorporate into the Study, two main criteria were utilized: the record of available evidence from previous studies, such as the Indianapolis Study, and those factors not previously studied in connection with fertility which appear plausible on various

theoretical grounds. A considerable number of hypotheses have been elicited from both sources. To describe in detail the rationales and theoretical derivations of each hypothesis would consume more space than we have available. The substantive theory underlying this Study has been described at length in a recent article.[4]

The primary theme which runs through most of the hypotheses is that a particular pattern of fertility performance and control depends on the extent to which the third child (for couples in the present study) is compatible with other life values and interests. Although at the level of a truism, this assumption serves to bring into prominence the notion that a fertility choice (whether implicit or explicit) involves a "cost" for the individual and the family, that is, certain desires and interests are either yielded or compromised in preference to others. (Decisions not to have children are as "costly," in a sense, as decisions to have them.)

The independent variables have been organized into three major categories so as to permit a description of the major elements of the concrete situation within which a fertility event takes place. The basic principle of organization is to classify separately those variables which refer to events which are intra-indiivdual (Person variables), inter-individual or intra-group (Family Group variables), and inter-group (Social Settings variables).

Two major assumptions underlie the hypotheses organized under the Person variables. The first is that excessive and unsatisfied needs for emotional support from other persons will tend to be incompatible with desires for additional children. This hypothesis rests on the assumption that children require both affection and support from their parents, and that individuals who themselves have strong needs in this direction will find parenthood a difficult and uncomfortable role. The second premise is that personal involvement in or identification with an extra-familial social role will tend to be incompatible with desires for further children. This assumes that parenthood becomes less important if other social roles can locate satisfactorily an individual's place in his social world; it further assumes that parenthood tends to restrict participation in other social roles.

The types of variables included under this area are emotional stability, level of economic anxiety, needs to give and receive affection, impulsivity, intelligence, orientation toward work, socio-economic mobility aspirations for self and children, satisfaction with familial roles and general affective attitudes toward children.

The major theme underlying the Family Group variables concerns the extent to which the social organization of the family group will be able to absorb adequately the stresses and strains of an additional child. Variables such as the husband-wife division of labor, the responsibility and control over family planning and child care, marital adjustment, and the impact of previous children on family and extra-familial activities are to be included here.

The third area, the Social Settings of the Family, is focused on factors operating through the direct interaction of family members with members of various social groups. The social milieu is conceived to set both the means and conditions of behavior. Three general considerations have influenced many of the specific formulations. They are (a) the relative benevolence of the environment; (b) the relative stability of the social settings with which family members are involved; and (c) the degree of control which is available to the family over the type and extent of involvement with its various social settings. Typical of variables in this area are inter- and intra-generational mobility (occupational

[4]Elliot G. Mishler and Charles F. Westoff, "A Proposal for Research on Social-Psychological Factors Affecting Fertility," in *Current Research in Human Fertility* (Milbank Memorial Fund, New York, 1955), pp. 121-150.

and financial, as well as residential), the perceived benevolence of the environment, the stability and continuity of the social groups with which family members interact, job security, general socio-economic status, rural-urban background and religious preference.

Items on the attitudinal components of the variables in these three areas constitute the second and main section of the interview schedule. The final portion of the schedule consists of factual questions on education, occupation, income, religion and the like, as well as the more conventional background items such as duration of marriage.

The Pretesting Program

In the development of measures of these independent variables, considerable attention has been devoted to devising sets of items that can be tested for scalability. Some pretest work in this connection has been undertaken already and some is currently in process. One of the methodological innovations for this study, incidentally, is the scale analysis currently underway on eight personality characteristics measured by the sentence completion technique. It is still too early, however, to evaluate the success of this particular work.

We anticipate that the construction of the preliminary schedule will be completed in the immediate future. Following this, a dozen or so interviews will be conducted, simply in order to examine the communicability and plausibility of the questions, as well as the schedule format. Presumably this will entail some revisions. The next step will be to draw a sample of about 100 couples that will approximate the characteristics of the final sample. This second stage of pretesting will permit an examination of the distributions of responses to the items, tests of scalability where appropriate, a test of sampling procedures, and a further evaluation of the interview schedule itself. For reasons of efficiency and economy we are planning to utilize the resources of one of the several survey research organizations that have had some experience in this general type of interview study.

Our current time estimates are that we should be prepared to start the major fieldwork operation within one year from now. Shortly thereafter we expect to publish at least one report that will summarize our pretest experience.

HEREDITY COUNSELING

Starting A Heredity Clinic

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HOW CAN THE GAP be filled between the present inadequate number of heredity counseling centers in this country and the 100 needed? With proper planning, budget, and personnel, an ideal counseling clinic can be brought into existence in a very short time; even with limited funds, a counseling service may be started with the hope that it will evolve from a limited functioning level to the ideal clinic.

Need for New Centers

The experiences of the present counseling centers show the need in genetic counseling that is not being met in the many communities without such centers.

Sheldon Reed (1953), director of The Dight Institute which is one of the oldest and most experienced heredity counseling centers in America, believes that a heredity clinic should be located in each medical center, one in each of the larger cities, and at least one in each State, making a total of about 100. This same author (1949) calls attention to the fact that the counseling center in Copenhagen saw over 1,000 cases in 1947 (ten times the number seen in 1938) from a supporting population of about two million. The Copenhagen center which the writer was privileged to visit in 1953 is older and has had more experience than any of the clinics in the United States. Their services since 1947 have not only been maintained but have continued to expand.

The need and value of genetic counseling has been emphasized recently by Böök (1955), Herndon (1954, 1955a), Reed (1954, 1955), Guttmacher (1954), Neel and Schull (1954), Stephens (1954) and Stone (1955). Ruth and Edward Brecher (1955) were able to find only 14 major heredity clinics and genetic counseling centers in the United States and Canada when they made their survey this year. At least six others are known to exist according to the first and only book (Reed, 1955) published on counseling in medical genetics. This book should be in the hands of every physician, to whom it is directed, because it will not only help him to avoid many of the most serious errors that are often made in giving counseling advice but it also will acquaint him with the kind of service he can expect from a genetic counseling center.

Most physicians do not have the time or the training to prepare and to interpret a complete pedigree chart for a family. Ideally, a pedigree chart should include information not only about children and siblings of the patient but also information about all siblings and all descendants of the four grandparents, and at least the maiden names of all four great-grandmothers. It is often most rewarding to be able to trace all descendants of the siblings of grandparents, and even great-grandparents. In many cases certain medical records of deceased and living members need to be checked whether or not it is possible to examine other members of the family. Interviews with representatives of various branches of a family are often necessary. Emphasizing the limited opportunity physicians have had to learn about medical genetics, a recent survey conducted by Herndon (1955) shows that 45 per cent of the medical graduates in the United States in 1952 still received no formal training in medical genetics; and the total instruction given during the entire four years at

the remaining schools was usually less than five clock hours.

Requirements

The requirements for an ideal counseling clinic may be described in terms of location, space and facilities, personnel, budget, and source of funds. It should be noted that much counseling of equal quality can be done in a setting quite short of the ideal to be described, but the extent of the service and the degree of convenience enjoyed by both personnel and recipient will be determined by how well these requirements are fulfilled.

A counseling center is best located in the medical school of a university which also offers graduate work. Such a location makes it convenient to give instruction in medical genetics, to give counsel on genetic conditions presented in the various clinics, and to conduct research in human genetics. The facilities of the various laboratories of a medical school and the clinics at a large hospital are necessary for special examinations. Graduates who have learned about a counseling service while in medical school are the most important group to refer new and urgent cases to the center.

The space needed for an ideal center will include waiting room, conference room, medical examination room, facilities for blood typing, electrophoresis, chromatography, finger- and palm-printing, anthropometry, photography, offices for staff members and graduate students, and library and seminar room.

The personnel should include at least one person trained in human genetics, a physician, a biochemist, secretaries, and laboratory technicians. In addition to salaries, equipment, supplies, and telephone service including long distance, the budget should include travel funds for relatives who are asked to come for examinations and travel funds for staff members who must visit relatives in their homes.

Funds for a counseling clinic may be secured in a number of different ways. The building and facilities of the University Institute for Human Genetics in Copenhagen was made possible by a gift from the Rockefeller Foundation. The Dight Institute at the University of Minnesota was made possible by a gift from Charles Fremont Dight, a physician who taught medical students most of his life at a salary seldom exceeding $1,500 a year. Dr. Dight's frugal living and wise investment created the estate which made his generous bequest possible. Unlike the centers at the University of Copenhagen and the University of Michigan, each of which is located in its own building, the Dight Institute is housed in one of the main campus buildings. Just as a pioneer work in most types of health and educational endeavors, even elementary education, had its original support from private sources, it would seem that private gifts must continue to be of primary importance in the development of an ideal heredity clinic.

Another source of funds is proposed by Dice (1952). He suggests that the cost of providing genetic advice be borne by the State just as in other projects that affect public health. Our state agricultural experiment stations and the U.S. Department of Agriculture perform a valuable research and counseling service which is free to farmers and stock breeders. Now that responsibility for the best possible development of strains of corn, chickens, and pigs is accepted by the State, he believes the State should also provide centers where information can be secured about the probability that a given genetic condition will be repeated in the offspring of certain individuals of a family.

How One Center Started

However desirable it may be to develop an ideal heredity counseling clinic spontaneously, most new centers will probably progress only gradually into this status. The clinic may not even have a budget of its own at the start. It should be emphasized that the reason for presenting some

of the methods used to develop a functioning center at Tulane University is not because they represent the ideal way to begin in every community but rather because they reveal one way which seems to have worked successfully in this community. The comments are presented in the hope that they may be helpful in bringing other centers into existence to meet the needs of the families of the 1,000,000 Americans reported by Neel (1951) to be afflicted with some kind of clear-cut genetic disease.

Of first importance in the development of the center at Tulane University School of Medicine has been the spadework of a professor of anatomy who over the years has emphasized the role of genetics to his medical students in such a way that increasing numbers later write him for counsel when faced with genetic problems. When the demand for lectures in human genetics and requests for counsel became too great in relation to other regular duties, he paved the way for the appointment in his department of a geneticist with primary interest and a Ph.D. in human genetics. The present incumbent, the writer, has an appointment on the visiting staff of a large hospital as Scientist, Division of Pediatrics, which gives him access to the record library, the various clinics, and conferences where counseling problems arise. An appointment as Genetic Consultant provides a working relationship at another hospital. An appointment on the Graduate Faculty provides the opportunity to teach and direct graduate students.

With office and conference room, waiting room, seminar room, secretaries, record files, photographic equipment and darkroom, and certain laboratory facilities provided by the Department of Anatomy, and with the facilities of the entire University seemingly available to round out the remaining requirements, the needs are met for heredity counseling and research. Numerous examples of cooperation could be cited.

The departments of Hematology in both the Tulane and the Louisiana State University Schools of Medicine were equally anxious that their facilities be used in typing blood for ABO, MNS, Rh, P, Duffy, Kell, Kidd, Lewis, etc. In one of the hospitals a biochemist will do the electrophoretic and chromatographic analyses on any specimens collected from families.

Although an ideal center should have a separate staff which includes a person with a Ph.D. in human genetics and a person with an M.D., one individual might combine these qualifications. The possibility of beginning with a geneticist who does not have the medical degree is emphasized here because there may be hesitation to develop a genetic counseling service without an M.D. director who has both an adequate training and a primary interest in human genetics. In medical schools and large hospitals a working relation with the widely diversified members of their staffs, who are available and willing to cooperate in the various examinations which only an M.D. can perform, would seem to make medical training for the director of the center somewhat less important.

Even when a university can provide only a salary and working facilities, the appointee can furnish his own transportation in the beginning, draw his own pedigree charts, and be his own technician and photographer. Funds secured directly for the counseling center and funds secured for special research projects in human genetics should soon parallel the ever-increasing demands for the service of the center. No attempt has been made at Tulane to publicize the free counseling service or to solicit cases, but word gets around. As noted by others, the referrals from physicians are by far the most stable and important source of the demand for counseling service, but the response from articles in newspapers and popular magazines, such as the one by the Brechers (1955), certainly results in an increased number of requests.

Aid in Diagnosis

Several examples have been published recently to illustrate how a genetic counselor can help an individual with a specific genetic disease or aid the relatives. The following case is an example of a service to an ophthalmologist who had made a complete examination of his patient but did not have time to examine any of the relatives.

A letter written by the mother-in-law of Mrs. X stated that an article about the genetic counseling service had been seen in the local newspaper. A prognosis of eventual blindness had just been given to Mrs. X, the mother of four children. The family wanted to know if the "awful blindness already present in six out of twelve siblings" of Mrs. X could possibly occur in the present or future offspring of Mrs. X. An examination of Mrs. X's hospital record and a conference with her doctor gave no more specific diagnosis than myopic retinal degeneration (a condition which may occur in some 20 different genetic diseases of the retina). A diagnosis of retinitis pigmentosa (a condition which occurs in a half dozen genetic diseases of the retina) was found on the hospital record of an older blind sibling. Finally, the medical record of the oldest blind sibling showed a diagnosis of Laurence-Moon-Biedl syndrome which has always been observed to be caused by an autosomal recessive gene. The pedigree of this family was compatible with this diagnosis. No other cases of blindness could be found in 52 relatives of these siblings, and the parents were third cousins. Although obesity and polydactyly did not happen to be present in Mrs. X, they were certainly present in some of her affected siblings. Not only was this family delighted to learn that blindness would not be expected to occur in the children of Mrs. X, but her doctor was glad to be able to make a specific diagnosis which could not be made directly from the eye examination of his patient.

Conclusions

A gap needs to be filled between the inadequate number of existing genetic counseling clinics and the number needed. The requirements for developing a new center are not beyond the reach of the many communities which have none available at the present time. These experiences at the Tulane University Center are presented to show one way of bringing a center into operation.

From the viewpoint of a University, the genetic counseling activities are a service to the community which are compatible with the best in public relations. From the standpoint of the individual (in nearly every family there is at least one gene for some undesirable condition), the service provides the best available answers to genetic problems. From the standpoint of the practicing physician, the service provides information which he does not have the time and in most cases the training to give. A donor can create a memorial in this field that will bring happiness to the present generation and elicit even greater appreciation from the generations yet to be born. From the standpoint of the geneticist, research opportunities for the pursuit of more knowledge about the genetics of various conditions go hand in hand with genetic counseling.

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BOOK REVIEWS

CULTURE AND HUMAN FERTILITY: Frank Lorimer *et al.*, Educational, Scientific, and Cultural Organization, Zurich, 1954, 510 pp.

In this important book the relation of fertility to social organization is studied for nonindustrial and preindustrial societies. The book's 500 pages are evenly divided between five case studies and a long theoretical statement by Professor Lorimer. The empirical materials marshalled by Lorimer are impressive, exceeding in scope those of any previous study. Likewise his empirical generalizations go further than earlier formulations.

While bringing credit to the author, these achievements reflect opportunities that did not exist ten, or even five, years ago. It is no accident that demographic theory has devoted so much attention to Western and industrialized nations. If it has neglected fertility differences among preliterates and preindustrial peoples, it is partly because few of these societies have produced useful census data. Nor in the past have anthropological data provided the demographer with much scope for theorizing. In 1922 Carr-Saunders used anthropological materials to gain a rough idea of the number of societies practicing infanticide, abortion, continence during the nursing period, and the like. However, rarely did his data permit him to compute a fertility rate or reliably judge the frequencies of a practice in societies where it reputedly occurred. Since then, C. S. Ford has deftly analyzed 60 preliterate societies from the files of the Cross-cultural Survey at Yale; and more recently Kingsley Davis has been schematizing the insights gained from this type of research. But although these studies have listed practices affecting fertility, specified the direction of their effects, and roughly gauged their widespreadness, they have not been able to quantify these effects or relate them to measured levels of fertility.

Fortunately case studies are accumulating which permit such quantification. This trend, made possible by advances in survey technique, will doubtless continue at a rapid rate. An important merit of the book under review is showing that this accumulation has already proceeded far.

In Part Five of the book (pp. 405-501) Giorgio Mortara provides a splendidly succinct and informative analysis of Brazilian fertility, based on a series of investigations carried out by the Instituto Brasileiro de Geografia e Estatística. Leaving aside Brazil's two largest cities, the picture presented is one of very little birth control and a high birth rate—42 to 44 per 1000 population—whose variation depends chiefly on the proportions marrying and ages at marriage. The predominating factor is not group differences in contraception, with the result that an entirely different rationale than the customary one for Western fertility is demanded.

In Part Two (pp. 253-338), Meyer Fortes reports about a field survey made in a rural community of Gold Coast Ashanti. The results portray a matrilineal society equipped for high fertility both by health and social structure. Non-rampant disease is implied by an average of more than 6 pregnancies for women over 40. Anti-natal practices hardly exist. Virtually all fecund women are married by their twentieth year. Impotency or barrenness give grounds for divorce while large families are idealized. Even a considerable degree of marital instability does not seriously lower reproduction since women, with access to lovers, remarry quickly and can count on a friendly environment in which to rear their offspring.

Fortes hypothesizes that even in an urban setting with more education and later marriage, fertility would not tangibly drop

provided that "the corporate organization of the lineage remains as important in Ashanti social life" as found in the survey. K. A. Busia's comparison of Gold Coast fertility under urban, small town, and rural conditions, reported in Part III (pp. 343-349), does not test this hypothesis directly. Fertility differences are small in the older age groups but among the younger women, urban fertility lags. These differences sharpen when women are classified by amount of education. Unfortunately Busia does not discuss whether the lineage appears to be losing its corporate character in large towns and among the educated.

In Part Four, A. I. Richards and P. Reining report on two fertility surveys undertaken by the East African Institute of Social Research in Buganda and Buhaya. Their results, though incomplete, offer an instructive contrast with those of Fortes. The Baganda and Bahaya are patrilineal peoples and although their lineages have lost much of their corporate character, their social structures appear well suited for high fertility. Women marry young. Children are prized and anti-natal practices are seemingly negligible. Here too the effects of marital instability are minimized by consorts, speedy remarriage, and provision for births outside of marriage. Notwithstanding, the Bahaya and Baganda exhibit completed fertilities of less than 4 live births as compared with approximately 6 for the Ashanti. Mindful of the very high rates of sterility — more than 30 per cent of the women over 35 are childless — the authors conclude that physical factors are primarily responsible for the low fertility. If so, here is proof that a comprehensive theory of fertility variation among nonindustrial societies cannot focus just on kinship values or structure but must consider determinants of sterility as well.

Only three aspects of Lorimer's long statement will be considered. To show that broader theories of fertility are needed, Lorimer assembles an imposing diversity of case studies. Several examples of unilineal societies with high fertility are described. The Pacific Islands are drawn upon for examples of preliterate societies which, to stay within fixed resources, have adopted birth control. Agrarian societies receive consideration. Attention is paid to societies in their initial steps toward industrialization. Of special interest is a section dealing with the fertility of preliterates when exposed to foreign influences. Frequently the effects exhibit two stages: a stage of "cultural shock" with declines in fertility brought about by disease, avoided or delayed marriage, marital instability, and seasonal labor migrations; and secondly a stage of "cultural accomodation," often characterized by high natural increases owing to the resumption of orderly marital relations together with a Westernized program of public health.

For the most part Lorimer is reluctant to generalize beyond his data and presents his hypotheses with great tentativeness. The outstanding exception is his theory that corporate lineages motivate high fertility. He observes that corporate kinship groups, such as lineages or clans, usually are competing militarily or economically and that some of their interest in high fertility derives from this context of intergroup competition. Hence their support of childbearing is more "compulsive" than is typical for extended or joint families.

Probably the most serious shortcoming of Lorimer's theory is its inattention to operational aspects. Nowhere does Lorimer discuss how the "motivating of high fertility" is to be measured. Ought its operational definition be a net reproduction rate, a rate of children ever born, the absence of anti-natal practices, a contempt for barrenness, or the idealization of large family size? All receive mention in the text. Yet all these indices vary in their relationships with kinship structure.

For example, any rate pertaining to children ever born is influenced by sterility and, as Lorimer documents, sterility may rise sharply after contact with outsiders.

Accordingly if completed fertility is the criterion, much depends on whether increases in sterility occur only when the lineage weakens. Furthermore, fertility comparisons between lineage systems and joint family systems are not valid unless sterility levels are similar. The net reproduction rate makes the relationship between criterion and kinship structure even more indirect. In situations of foreign contact, mortality may be more immediately affected than fertility.

The suitableness of another criterion, presence or absence of anti-natal practices, may be doubted in view of Ford's demonstration that every society practices birth control under at least some circumstances. Use of this criterion requires classifying conditions felt to justify birth control and then measuring the frequency of anti-natal practices within these conditions. To use attitudes toward barrenness as the criterion means losing the distinction between idealizing large and moderate-sized families. Probably in any society attitudes toward barrenness lean toward pity or contempt, thereby necessitating the measurement of degree. Moreover, the degree of contempt or pity is not independent of the sterility level. In a society so disease-ridden that 30 per cent are sterile, barren women cannot be so thoroughly discriminated against as in a society where childlessness is rare.

Another index of "motivating high fertility" is attitudes toward ideal family size. This criterion involves the problem that desiring a large family has very different meaning depending whether its attainment is likely or not. Nevertheless, this is prehaps the most relevant criterion. Unfortunately there are currently far too little data on these attitudes to provide a test of Lorimer's theory.

Lorimer also develops a model for estimating "absolute" biological capacity for procreation." He concludes that an average of 8.3 live births is the physical ceiling for women who survive their childbearing years. His model assumes that a fecund woman is capable of one live birth per 2.8 years. The proportion fecund at any given age is derived from three curves representing "maturation of procreative capacity," freedom from fecundity impairments, and priorness to cessation of ovulation.

Models of this sort are enormously important. A theory for explaining fertility variation among preliterate and preindustrial societies cannot go very far without an apparatus for evaluating factors such as age of marriage, duration of nursing, continence during nursing, time spent in marriage classified by age, nutrition, and incidence of disease. Only a versatile model of fecundity gives this apparatus. The fertility gained by early marriage is conditioned by the length of "adolescent sterility." The impact of a prolonged nursing period depends on its extending the period of postpartum amenorrhea and anovulatory cycles. If the ratio of this period to the total nursing period is high, then the importance of continence during lactation has been overrated. Inasmuch as fecundity declines with advancing age, the impact of marital instability is determined partly by whether it falls heaviest on the twenties, thirties, or forties. The relation of nutrition and disease to fertility may be roughly indexed by the proportion involuntarily childless and it might be possible to derive empirical relationships between this proportion and the proportions unable to have more than a specified number of children.

Lorimer's model tells something about the relation of fertility to marriage age. Its sensible results stamp it as a useful first step. One can only complain that it does not go far enough. The needs outlined above require a more versatile model, particularly one so formulated that it responds to assumptions about sterility and duration of nursing.

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THE TREND AND PATTERN OF FERTILITY IN GREAT BRITAIN. A Report of the Family Census of 1946: D. V. Glass and E. Grebenik, Her Majesty's Stationery Office, London, 1954, Pt. I, 306 pp., Pt. II, 253 pp. (Papers of the Royal Commission on Population, Vol. VI.)

A relatively recent development which is contributing greatly to a better understanding of fertility trends and differentials in certain countries of the Western World is the study of the fertility of actual cohorts of women (*e.g.*, of the women born or married in a given year). The present work of Drs. Glass and Grebenik is a noteworthy contribution in this field, for it is undoubtedly the best report published to date on trends and differentials in the fertility of marriage cohorts of women. It is based mainly on the Family Census taken in 1946, utilizes data from the Family Census of 1911 in some of the long-time comparisons, and is brought relatively up to date by means of estimates based on births registered during 1946 to 1950. This body of fertility data probably is not surpassed in any other country, although Japan, Sweden, and the United States have collected substantial amounts. Certainly no other report has yet appeared which contains such a careful and comprehensive analysis of this type of data.

The Family Census of 1946 was undertaken for the Royal Commission on Population, which was established before the postwar upsurge of the birth rate had occurred, and while the nation was concerned over the possibility that the population would cease to grow, and might even begin to decrease in numbers, within a few years. Fertility was known to have declined substantially since 1911—the date of the last preceding family census—but the trends and differentials could not be determined adequately from annual vital statistics because the amount of information collected was too limited, especially prior to 1938. The best way of obtaining the desired facts seemed to be through a census of a 10 per cent random sample of ever married women, asking questions relating to (a) dates of the woman's birth, her first marriage, the termination of her first marriage, and the birth of each of her children; (b) whether her first marriage ended by death or divorce; and (c) her first husband's occupation, his status (employer, self employed, salaried employee, or other employee), and his employer's business. Schedules were mailed to the women in the sample, enumerators called to collect the schedules, and women not found at home were sent followup letters. Replies were finally obtained from more than 87 per cent of the women in the sample, which is a high proportion for a voluntary inquiry.

Because the census had to be taken by an *ad hoc* organization when manpower was very scarce, because it related to a sample based on ration books exchanged in 1945, and because women could not be required to reply (and some refused to do so), questions arise as to enumeration procedures and sampling problems. These are discussed in detail in Chapters 1 to 5 and their appendices. The authors emphasize that the original data were biased in certain ways, *e.g.*, the response rate was higher in some parts of the nation than others, and both recent marriages and childless marriages tended to be under-represented. Adjustments for area differences in response rate were made relatively easily by withdrawing or duplicating the proper numbers of punch cards. Adjustments for the shortages of recent marriages and childless marriages, and for certain other biases, were more complicated, but were made nevertheless even though in some cases the change in results was very small. Unadjusted data, and partially and completely adjusted data are presented in some of the tables to help the reader. The various tests and checks which were made indicate strongly that these data, while not perfect, are highly reliable, and provide an adequate basis for the analysis.

Two analytical chapters (with their appendices) comprise three-fourths of Part I.

Chapter 6 is devoted to marriages of completed fertility (occurring before 1925); these and later marriages are considered in Chapter 7. Most of the discussion relates to women whose first marriage was performed before age 45 and lasted until age 45 or the census date. Because these women constituted such a large proportion of all the women in the sample who were married before 45, there is only a slight difference between their fertility and that of the entire group.

Average marital fertility is shown to have dropped from 5.8 births per marriage of 1870-79 to 2.21 for those of 1925, reflecting in part a doubling of the proportion of childless marriages but in greater degree a large decrease in the proportion of fertile marriages with more than two children. There was a tendency toward greater concentration of childbearing in the first 5 or 10 years of married life, for the decline in the average number of births per fertile woman more than balanced the increase in the average number of months between marriage and the first birth, the second, etc. (The latter has been quite stable, however, for women with a given size of completed family.) In other words, the decrease in fertility rates has been larger during the second five years after marriage than the first, during the third than the second, etc.

Attention is called to the tendency for the relative reduction in fertility from the marriages of 1900–09 to those of 1925 to be the same for wives in each age-at-marriage group, and for wives of non-manual workers and of manual workers. There was little change in the average age of brides prior to 1925, and *little narrowing of occupational differentials in fertility.* The latter vary substantially with duration of marriage, being smallest during the second year and increasing during subsequent years.

Because the date of birth of each child was obtained in the 1946 census (though not in that of 1911) it is possible to compare the fertility of recent marriages of specified duration with that of earlier marriages of the same duration for different age-at-marriage and occupational groups. One of the important facts brought out is the tendency of the decline in fertility during the first 10 years of marriage to "flatten" among the marriage cohorts of 1926 to 1940, and for fertility during the first 5 years to rise among the cohorts of 1940 to 1945. These trends are clearer for women married between ages 25 and 40 than at younger ages.

Attention is called to the effect of World War II on the timing of births, *e.g.*, variations between the cohorts of 1935 to 1940 with respect to fertility 2 to 5 years after marriage which practically disappeared by 10 years.

Since 1935 there has been a substantial decline in the previously stable average age of brides composing the annual marriage cohorts. At the same time there has been a reduction in the influence of age of bride on fertility during the first 5 years of marriage, the rate for marriages of women aged 25 to 40 moving closer to that of women aged 20–24. While for each marriage cohort the marriages at younger ages have been substantially more fertile than those at older ages, it is pointed out that this has reflected differences in social status and other conditions, and does not justify the conclusion that the recent decline in average age at marriage will lead to a considerable increase in marital fertility.

Projections of ultimate family size for marriages at ages under 45 are given for the cohorts of 1926 to 1943. They show little change among the cohorts of 1926 to 1941 and slightly higher fertility for those of 1942 and 1943. This small increase and the substantial decline in mortality are expected to raise the "replacement rate" from about .74 for the women *born* in 1895–99 to .94–.98 for those making up the *marriage* cohorts of 1941–43 (most of whom were born during 1911–23).

Part II of the report is devoted to detailed tables which should be of much value to many students for analytical purposes.

So much for content. The title of the re-

port is slightly misleading. A more accurate title would be "The Trend and Pattern of Marital Fertility in "Great Britain," for there is no discussion of changes in the proportion of women who married nor of their effect on the fertility of all women. Both may have been small, however. Some regional analysis might have been desirable, but this would have required the expansion of what already was a large undertaking. There is an excellent summary, but no list of the numerous text tables, no index, and few diagrams. The terminology seems clumsy in a few places, *e.g.*, "*n*-parity" could have been used conveniently in referring to women at a time when they have had births of *nth* order but not *nth*+1, and "parity-specific rates" would have described much more precisely than "true birth-order rates" the number of births of *nth* order during the year y per 1,000 women of *n*—1 parity in that year.

American demographers have grounds for deep concern over the fact that the inadequacy of marriage and divorce statistics during past years, and the absence from the birth certificate of a question on year of marriage, prevent the computation of fertility tables for marriage cohorts in the United States as has been done for birth cohorts. While each type of cohort table has much value, neither can take the place of the other. Under the circumstances more effort needs to be devoted to the analysis of the fertility of marriage cohorts on the basis of the information collected in the 1950 census, and to obtaining more adequate information in the 1960 census.

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Population Policies: A Survey of Recent Developments: Hope T. Eldridge, with a Foreword by D. V. Glass, International Union for the Scientific Study of Population, (Administrative Office: c/o The American University, Washington 16, D. C., U.S.A.) 1954, $2.00. vi + 153.

The 1954 World Population Conference considered almost every aspect of population under a large variety of headings, from abortion to underdeveloped areas. Underlying all of these seemingly diverse topics are basic questions of policy, of the influence of economic and social action, politically implemented, on population growth and distribution. Therefore, as Mr. Glass writes in the Foreword, ". . . it seemed desirable to make available to the Conference, as a background document, a survey of recent developments in some of the main areas of population policy," (p. iii). The present volume is the result of this survey.

Many different policy aspects exist and a selection had to be made. Accordingly three areas were delimited in conjunction with the committee of the International Population Union and UNESCO under whose auspices the survey was made. These are: the family and social security, international migration, and fertility control. Excluded from this survey are such aspects as natural resources, capital formation, technical assistance, energy resources, religious influences, and perhaps others. Although one might wish that these topics had been included, it is clear that given the limitations within which she had to operate, Dr. Eldridge did well in limiting herself to the first three areas. Certainly they are both complex and important enough to constitute a study in themselves.

This volume presents a compilation and ordering of official written policies and attitudes. An attempt was made to survey all independent governments throughout the world; dependencies, territories, etc., were omitted.

The first main topic, "The Family and Social Security" presents all available materials on the following sub-areas: constitutional provisions and international conventions on this subject, family allowances, protection of health and maternity, marriage grants and loans, and the protection of old

age. Dr. Eldridge recognizes that there are other types of welfare activities which may prove to be at least as powerful influences as any of those she discusses, but which had to be excluded. These include: child labor laws, compulsory education, conditions for employment of women, etc. Since almost any governmental decree in the field of social welfare may have demographic repercussions, there is no uniquely correct dividing line to separate out those factors which should be included or excluded insofar as a survey of population policy is concerned. Since lines had to be drawn, the specific topics included in this survey were those which are commonly believed to have the closest relation to population trends. Hence, the writer simply followed precedence and discussed those topics which have been generally discussed by other writers under the heading of "population policies."

A very complete summary of existing laws and regulations on the above mentioned sub-areas is presented. For example, we learn that of 39 national constitutions adopted since 1945, 33 acknowledge governmental obligations with respect to family welfare. The types and amounts of family allowance paid (or supposedly paid) are discussed in some detail; for example, the relation of family allowances for a three child family to per capita national income and average wages for a number of countries (p. 53) presents a very illuminating picture of the economic meaning of such allowances. Adequate references are given so that the reader can investigate any country in as much detail as he pleases.

The second main topic, "International Migrations" includes: national policies and programs, reciprocal and multilateral agreements, international programs and conventions, and objectives of national migration policies. International migration seems to be a multi-horned dilemma; the writer notes that ". . . national policies with respect to immigration or emigration are subject to frequent adjustment under the pressure of immediate circumstances or changing conditions. Longer range objectives often have to give way to short-term considerations (p. 94)."

The third main topic, "Fertility Control," includes a discussion of the problems of comparability and national laws and policies covering both abortion and contraception. Besides summarizing the official laws of the various countries, two very important points are noted by the writer. One is that the written law does not always reflect the policies actually in force. This is found both among high and low fertility countries and seems to bear little relation to the degree of interest which the country may have in the question of encouraging population growth. By extension, it may well be that the policies actually in force, in turn, bear little relation to the practices of the people; this is the reviewer's extension and not that of the writer.

A second important point noted is the basic dilemma faced—namely the choice between the ideal of individual freedom and a restriction of that freedom in the interests of the State. Obviously this dilemma is faced only by those countries which have a genuine interest in and which attempt to provide safeguards for the freedom of the individual.

Finally, in Chapter V, two other factors are "gone over once lightly"—marriage and divorce laws and the position of women.

No attempt is made to judge the demographic effects of these policies. For one, many of these policies have been on the books for too short a period of time to have produced any consequences, even if they are so capable. For another, the statistics which would be necessary to prove or disprove the efficacy of many of these policies, are lacking in whole or in part. Finally, it must be noted, that if policies are introduced and demographic changes consequently follow, it is still difficult to prove "cause and effect"; we have no way of knowing that such consequences would not have followed in the absence of the population policies. The United States seems to be a case in point;

in the almost complete absence of any official population policies, we persist in having demographic changes of such magnitude as to almost defy explanation, for the moment at least. At various points in the monograph, Dr. Eldridge intimates that statistical evaluation of these policies would be in order; because of the considerations mentioned above and the administrative limitations placed upon her survey, however, such evaluation had to be omitted.

This survey left two impressions with this reviewer which he feels are worth noting. The first is that a great many of these official statements are "for the record only." Some countries have a genuine interest in bringing about demographic changes, generally either a raising or lowering of their current fertility rates. Other countries, however, including some of the grossly underdeveloped ones, jumped on the bandwagon in an effort to prove that they are enlightened and progressive even though poor, and to convince their people that their government is looking after their welfare. For such countries, writing a high-sounding law into the books is easy enough; (indeed, a whole new Constitution can be drawn anytime it seems politically expedient). Dr. Eldridge only notes, "Any attempt to appraise the possible effects of family programs on general fertility must be approached with caution. Most schemes are still in evolution," (p. 76). This reviewer would be inclined to change the phrase "still in evolution" to "still to be implemented."

Another impression obtained by the reviewer is that the problem of bringing about demographic change may be far larger than demographers often visualize. It may be, as Dr. Eldridge suggests, that many other factors which she omitted from discussion have far more important population policy implications than those considered in this survey. For example, the role of cottage industries in under-developed countries is still being actively debated by the economists. It may well be that the demographic implications of cottage industries may far outweigh any economic considerations; perhaps the production of goods in such cottage and village industries is inseparable from the production of children.

Dr. Eldridge's survey is a notable contribution to the field of population policy as delimited here. In addition, it should be extremely useful in any university course in population. This reviewer agrees wholeheartedly with Mr. Glass (who has written much and well on population policies) in his emphasis that this "report provides an admirable starting point. It is to be hoped that she herself will take part in further work, the need for which is so clearly shown by her present survey" (p. iv).

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The Facts of Life: C. D. Darlington. The Macmillan Co., New York. 1955, 467 pages.

The name Darlington is a well-known and significant name in the field of genetics. There are few, if any, students of this subject who do not own a copy of his book "The Handling of Chromosomes," (written with La Cour), and who have not been repeatedly stimulated by the imaginative work of this author. Rarely orthodox in his views, and often controversial, Darlington must still be recognized, even by his most orthodox and severe critics, to have been the author of many important concepts in the fields of genetics and cytology. Probably Darlington's most important contribution is that his highly original thinking has led many other workers into productive investigation in every phase of these two sciences. It is fair to predict that the original and provocative views presented in "The Facts of Life" will prove to be of a similar importance to the social sciences. There may be some who will be offended by his discussion of the

Virgin Birth, others antagonized by his views on class and race, or a plasmagene solution to so many of the problems in human genetics. It is with these and similar issued posed in this book that the test will come, not of Darlington, but of his critics. By posing his explanations of race and society, sex and reproduction, and almost every other conceivable problem facing a science of man, Darlington has forcefully and often eloquently formulated the problems to be attacked. The existence of these problems cannot be disputed, and to disagree intelligently with the explanations offered will require thought; if the disagreement is pursued constructively, it will lead to a far greater knowledge concerning these vital topics. Darlington achieves his forceful style of writing by striking statements and similes, which at times affords almost terrifying opportunity for exploitation by the unscrupulous. As certainly as this book's greatest danger lies in statements being taken out of context, so does its greatest vulnerability to prejudiced or suppressive criticism within the biological and social sciences.

The title of this book is meant to imply the popular connotation of "the facts of life." In a historical sketch of the evolution of ideas concerning reproduction and heredity a meaning is evolved which could be termed "the facts of life" for science as well. The facts and fancies, the errors in reasoning, and consequences of failure to distinguish between biology and morals, and inductive and deductive reasoning are forcefully demonstrated by repeated illustrations from Aristotle down to Michurin and Lysenko. These illustrations are admirably summarized (p. 236) as follows:

> "The experiments . . ., were at first not faked at all. At first their errors were largely due to accidents in handling and in interpretation. Such accidents are made easy by the experimenters' ignorance of the standard methods of conducting experiments, their ignorance of the results obtained by other people from experiments, and their overpowering desire to reach a conclusion which they are certain is the right conclusion, the morally right conclusion, and therefore the only conclusion that any honest man can reach."*

It should be pointed out that Darlington himself is quite human and comes dangerously close on several occasions to arriving at a conclusion which he apparently is "certain is the right conclusion," e.g., (p. 257), the stress placed on the male role in identical twinning to give strength to his plasmagene explanation; (p. 320), the implied degree of decrepitude of the Tristan da Cunha and Pitcairn F_2.

The main purpose of the book is quite obviously to try to insert a foundation of modern genetics under the thinking of the social sciences. It is correctly pointed out that biology has been almost totally rejected by Anthropology and Sociology; this largely because it can be proved that Herbert Spencer was wrong when he presented his theories from a not too adequate knowledge of the biology even of his day. The author's intent is to formulate new approaches in social thinking in the light of modern population genetics, and he develops very well both the genetic importance of the individual, and of the population. Whether one agrees fully with his definition of classes and intra-populational groups as important breeding populations, or with his ideas concerning social structure, would seem unimportant, to this reviewer. This is a book of ideas for new and realistic biological approaches to supplant in the social scientists' consideration of biological factors, theories such as Spencer's "Survival of the Fittest," and Malthus' "Struggle for Existence," which as originally presented, took no account of the stock or the race, the group or the community, as real or biological entities, (p. 373).

Darlington expresses an interesting view concerning race and class which is quite

*Quotations from the text may seem excessively long. They may be, but with Darlington's style of writing it is often necessary to quote at length to avoid distortion of meaning.

original, and adds a rather constructive twist to the problem (p. 293):

> "One of the definable genetic characters and, as one would say, virtues, of the human races and classes (as also of individuals) is the capacity of social cooperation. For the co-operation of the genetically different groups permits the building up of more complex societies and cultures and the exploitation of the earth's resources by a more numerous population. And, what is most important of all, in the light of genetics, a genetically diversified population.
>
> Thus we arrive at a paradox which timidity usually hides from us. The assumption of a genetic basis for race and class differences provides the evidence, the only scientific evidence, in favor of racial tolerance and cooperation."

From this he proceeds to an adaptation of Gause's principle to human society:

> "Individuals and groups which are genetically similar are bound to compete with one another. Only when they are genetically different can help predominate. Whether they do help one another, of course, depends on heredity and education, but the education can be of no use if it does not depend on a recognition of the laws of nature."

Most geneticists and anthropologists will agree that different breeding populations may differ in gene frequencies for the blood group antigens. Darlington, having built up a case for genetic differences between groups — genetic differences affecting the manner of their adaptation to a social or cultural environment — is pleading for a social view based upon what he calls "the laws of nature." In other words, a social view which distinguishes between "morals and biology."

Much of Darlington's idea of man's adaptation to his social environment stems from the theory of mutual or double selection as presented by Davenport. From this it is possible to move on to the concept of a choice of environments which in turn is a hereditary characteristic (p. 303). These ideas culminate (p. 304) as follows:

> "Man is immensely adaptable, not through the plasticity of the individual but through the variability of the species. This variability disguises itself as individual plasticity through the skill and perseverance with which individuals of the myriad varying types, so far as they are mentally coherent, seek the environment which is right for each rather than attempt to fit themselves to the wrong one.
>
> Individual adaptability is indeed one of the great illusions of common-sense observation. It is an illusion responsible for some of the chief errors of political and economic administration today."

It is impossible in a review to touch upon even a fraction of the really stimulating ideas expressed in this book. The above must suffice, but it should be apparent even from this, the interest which this book should demand. The field of eugenics, which is still casting about for formulae for a positive program, should find Darlington pointing toward a multitude of inspirational ideas. This is not a book for the pure layman, but its principal challenge does not come in terms of technical background. The main challenge is in following Darlington's process of reasoning and distinguishing between disagreement based upon the readers' loss of the "trail," moral prejudice, or honest difference of interpretation of the "facts." This is an inspirational and important book which should stimulate a considerable amount of thinking and discussion between the biological and social sciences.

Richard H. Osborne
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PERIODICAL ABSTRACTS

GENETICS

Qualitative Aspects of the Population Problem. C. Stern. *Science* **121** (1955), pp. 683–686.

The author starts with the postulate that human beings are different from one another both genetically and culturally. Inasmuch as reproduction and migration are bound to occur unequally among different nations, classes, and types, the composition of world and national populations is sure to change culturally and probably genetically. For example, there will be a growing influence of Asiatic thought and culture with the relative expansion of Asian populations. At least one genetic change can be predicted from the intermingling of formerly isolated groups, namely, a general reduction in frequency of recessive hereditary diseases.

Even though socio-economic groups may be genetically different, low birth rates in the highest strata need not produce any long-term genetic effects. Such differential reproduction may be compatible with a stable population structure provided that low reproductive rates at the top of the socio-economic scale are balanced by similar rates at the bottom of the scale. However, this should not obscure the fact that populations could be maintained at least as well by a relative increase of fertility among the better-endowed, and such an increase would lead to genetic improvement with respect to the traits concerned. Although there is undeniable danger in interference with existing patterns of reproduction, "a do-nothing attitude also has its specific consequences, for better or worse. . . . Must we really, in the long run, renounce the benefits that come from positively selective reproduction, or should we rather learn to devise methods compatible with individual responsibility and voluntary action?" Little can be done along these lines during the present burst of population growth, but "when an equilibrium between births and deaths is approached again, mankind should be ready to wisely direct its reproductive patterns."

Intra-Uterine Selection by the ABO Incompatability of Mother and Fetus. Ei Matsunaga. *American Journal of Human Genetics* **7** (1955), pp. 66–71.

Incompatibility between parents with respect to the ABO blood groups may be a major cause of spontaneous abortions. When 2709 Japanese families were classified according to blood groups of the parents, it was found that group O mothers with group A husbands had 14 per cent fewer group A children than expected from genetic theory. Similarly, O mothers with B husbands had 10 per cent fewer B children than expected. In matings of these two types there is a statistically significant excess of miscarriages in comparison with other matings, and a deficiency of recognized pregnancies that may represent additional, very early miscarriages.

(*Erythroblastosis of newborn infants results when, during pregnancy, the mother's blood acquires antibodies against a foreign blood factor, usually one of the Rh factors, which the fetus has inherited from its father. In the case of the Rh factors, the antibodies do not usually reach significantly high levels until late in the second or third pregnancy. Every mother of blood group O, however, already has antibodies against group A or B blood before her first pregnancy, so that theoretically her antibodies could injure an A or B fetus at a very early stage of development.*)

Familial Incidence of Craniostenosis. L. A. French and R. L. Suechting. *American Journal of Diseases of Children* **89** (1955), pp. 486–488.

Closure of the spaces between the bones of the skull in early infancy may restrict growth of the brain and produce mental subnormality. The cause of this condition is unknown, but early recognition and surgical treatment may prevent mental impairment. The present report describes a boy who was first treated for craniostenosis at the age of 11 months, but whose subsequent mental development revealed irreversible brain damage and an I.Q of 61. When a baby sister was born into the family, the mother insisted upon X-ray examinations of the skull and at 6 weeks craniostenosis was recognized in an early stage. Treatment was successful and at the present age of 6 years her physical and mental development are normal.

Genetic Study of Microcephaly Based on Japanese Material. Taku Komai *et al.* *American Journal of Human Genetics* **7** (1955), pp. 51–65.

The authors found 78 families containing 143 cases of microcephaly either examined by them

or described in reliable Japanese reports. Seven additional microcephalics born in Hiroshima soon after the atomic blast were excluded as probably due to direct effects of irradiation during development. None of the remaining mothers had been exposed to radiation. A rigorous analysis of the 78 families indicated that the condition is probably a simple recessive hereditary trait. Although 65 per cent of the cases were males, the excess was apparently due to more complete reporting of males. A relatively high incidence of mental retardation was found among the relatives with normal heads, but apparently most persons who carry a single gene for microcephaly have normal intelligence. Five of the microcephalics themselves had children, all normal. For this recessive gene the estimated frequency of carriers in the Japanese population is between 1/80 and 1/150, just as in Sweden, and there appear to be about three new mutations in every 20,000 persons.

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POPULATION

Age-specific mortality in selected Western European countries with particular emphasis on the nineteenth century. Odin W. Anderson. *Bulletin of the History of Medicine,* **29** (1955), pp. 239-252.

The author advances the thesis that declines in crude (total) death rates antedated declines in infant mortality in countries of Western civilization by 10-90 years. According to him the "percent improvement" in death rates during the first half of the 19th century tended to be very low for children under one year of age. The early declines in mortality were due mainly to improvements in nutrition rather than to medical advances.

One criticism of the data adduced by the author is that the dates of "onset of fall" in both crude and infant mortality rates are much later than those commonly seen for the countries of Western Europe. For instance, 1870-1880 is given as the "onset of fall" in crude mortality rates in England and Wales. Actually it was during this decade (1876) that the birth rates in England and Wales began to fall and this country is frequently used as the classic example of the "demographic gap" in that the decline in the death rate presumably began a century or more earlier.

Historical demographic data can be very tricky. One possible pitfall is that of assuming that higher mortality rates than the earliest *recorded* ones did not exist. Another is that of ignoring the possibility that declines in mortality may be masked by secular improvements in registration. On the other hand, it must also be admitted that if by "date of onset of decline" is meant the initiation of a *sustained* decline, such dates may be considerably later than those heretofore assumed by historical demographers.

John Maynard Keynes' theories of population and the concept of optimum. William Peterson. *Population Studies* **7** (1955) pp. 228-246.

Three broad sets of factors, the post-war increase in the birth rate in western countries, the post-war decrease in the death rate in Asiatic countries, and the general interest in technical assistance to underdeveloped areas, have conspired to stimulate a revival of interest in Malthus. It is therefore of interest to read Peterson's account of J. Keynes' theories, for it is largely the story of the manner in which Western economists and demographers shifted from fear of overpopulation in the twenties to fear of underpopulation in the thirties.

Keynes participated in the revival of Malthus during the twenties. He apparently was never bothered much by the implicit contradiction of his shift to the "underpopulationist theory" in his later years.

Primarily an economist, Keynes wrote very little on population *per se*. However, his frequent references to population in his writings on other subjects and the works of his disciples in the field of population give him prominent place as a demographic theorist. A disciple who represented Keynes during the pro-Malthus stage of his life was Harold Wright, author of *Population* (Cambridge, 1923). W. B. Reddaway represented Keynes as an underpopulation theorist, particularly in the first edition of his *Economics of a Declining Population* (London, 1939). Reddaway's counterpart in the United States was Alvin H. Hansen, author of *Economic Progress and Declining Population Growth* (*American Economic Review,* March, 1939).

The adjustment of married offspring to their parents. Sheldon Stryker. *American Sociological Review* **20** (1955), pp. 149-154.

This investigation is a part of a larger study of the relations of married offspring to their parents. The chief purpose of the present article is to report on the reliability of an "index of married offspring—parent adjust-

ment." The adjustment index was based upon items "subsumable under the four categories: affection, intimacy, tension and sympathy (ego involvement). From a total of about 150 items, forty were selected (ten for each of the four categories) for the index. The selection was made on the basis of high agreement among ten judges as to relevance of the item and on the direction of the items relation to adjustment.

The author claims that the data indicate the following "(1) Adjustment to parent increases with dependence upon parent; (2) females are more likely to be dependent upon their mothers than are males; (3) husband's adjustment to in-laws is independent of wife's adjustment to her parents; (4) husband's adjustment to mother-in-law is negatively related to his wife's dependence upon her mother; (5) husband's adjustment to father-in-law is positively related to his wife's dependence upon her father; (6) the impact of the wife's dependency on husband's adjustment is specific to the parent on whom the wife is dependent; (7) females and males with children are better adjusted to their mothers than are those without children; (8) age of offspring and the number of years offspring have been married are unrelated to the adjustment of offspring to parents." (P. 154)

Since the total universe consisted of only 51 married men and 53 married women, the small size of *n*'s in the various subdivisions may be appreciated. The author systematically subjects all findings to tests of significance. He states: "The small number of cases involved should be pointed out, but perhaps more noteworthy is the consistency of the emergent pattern *in spite of* the small *n*'s." (P. 151). Probably, the author would also agree, however, that the eight results given above should be interpreted as highly tentative and as based upon a single trial with a very small sample.

Some aspects of the demographic situation in Egypt based on the stability of census age distributions. M. A. El-Badry. *The Milbank Memorial Fund Quarterly.* **3** (1955), pp. 268-305.

On the assumption that neither birth rates nor death rates for the total country of Egypt changed appreciably during the period 1907-1947 and under the further assumption that immigration and emigration were of negligible importance, Dr. Badry reasons that during this period Egypt had a virtually stable population. An hypothetical stable population flows from the conventional computation of life table values. In the present instance, the author reverses the process and computes life-table values under the assumption that a stable population already existed in actual fact in Egypt. To implement this assumption, however, the author found it necessary to carry out a substantial amount of preliminary adjustment of the age distribution because of misreporting of ages in the census. There was a considerable amount of "heaping" at ages divisible by five, presumably because of illiteracy and lack of precise knowledge of ages.

With the method used, El-Badry used age 5 as the radix of the life table. However, by resort to other data the expectations of life at birth for the whole 1907-1947 period were estimated at 31.8 years for males and 33.6 years for females.

The method developed is applicable to other underdeveloped areas which may lack adequate vital statistics if the conditions of a stable and closed population are met. However, such conditions are becoming rarer with the onset of marked declines in mortality in underdeveloped areas. The method could hardly be used for Egypt for a period more recent than 1947. Nevertheless, the method may be of value for a long time for purposes of historical demography.

The prospects for population forecasts. John Hajnal. *Journal of the American Statistical Association,* **50** (1955), pp. 309-322.

The author reviews some of the past successes and failures in forecasting population. He argues "(1) that population projections in the future as in the past will often be fairly wide of their mark—as often as simple guesses would be; (2) that, nevertheless, the frequent preparation of projections will continue; (3) that a projection can be useful as a piece of analysis even if its accuracy is low; (4) that simple unpretentious short-term projections should be used to meet most practical needs for population forecasts; (5) that greater flexibility and variety in techniques for births need to be developed." (p. 309.)

In his concluding paragraph the author states: "If there is a general lesson to be drawn from all this, it is, I think, first that as little forecasting as possible should be done, and second that, if a forecast (more elaborate than the quick calculation discussed earlier) is undertaken, it should involve less computation and more cogitation than has generally been applied. Forecasts should flow from the analysis of the past. Anyone who has not bothered with analysis should not forecast. The labor spent

in doing elaborate projections on a variety of assumptions by a ready-made technique would often be much better employed in a study of the past. Out of such a study may occasionally come important insights about unexpected possibilities in the future." (p. 321.) In the reviewer's opinion, few would deny that knowledge of the past is essential to intelligent opinions regarding the future. Yet it is also true that some of the projections of fertility and mortality which have gone awry are those which are based rather solidly on assumptions that the future will witness an orderly development of the past trends.

CLYDE V. KISER
Milbank Memorial Fund

First International Congress of Human Genetics
Copenhagen, August 1-6, 1956

Scientific Program

The First International Congress of Human Genetics will include plenary sessions, sections and exhibitions.

Plenary Sessions

Plenary Sessions will be held every morning from August 1st to 6th. Invited papers will be submitted and discussions held.

The subjects of the plenary sessions are planned to be:

WEDNESDAY, AUGUST 1ST:

1. Opening session.
2. Mutation in Man.
3. Radiation Genetics and Its Human Implications.

THURSDAY, AUGUST 2ND:

1. Quantitative Inheritance in Man.
2. Comparative and Experimental Pathology in Relation to Human Genetics.
3. Biochemical Genetics in Man.

FRIDAY, AUGUST 3RD:

1. Methods in Human Genetics.
   a. Family Studies.
   b. Twin Studies.

SATURDAY, AUGUST 4TH:

1. Genetics and Medical Research.
2. Medical Applications of Human Genetics.

MONDAY, AUGUST 6TH:

1. Social Applications of Human Genetics.
2. Epidemiological Control of Hereditary Diseases.
   Genetic-Hygienic Registration.
   Medico-Genetic Ascertainment.

Sections

In the afternoons of August 1st to 4th the meetings will be divided into *sections* during which invited and proffered papers will be read. The following sections have been planned in accordance with the subjects of the 220 papers proffered so far:

1) Population Genetics, Demography and Statistical Analysis.
2) Multifactor Inheritance in Man.
3) Biochemical Genetics in Man.
4) Anthropological Genetics Including Problems of Parentage and Identification. Physical Growth.
5) Cytological and Radiation Genetics in Man. Human Chromosomes.
6) Blood Groups and Immuno-Genetics in Man. Crossing-over and Linkage in Man.
7) Genetics and Psychiatry, Psychology and Criminology.
8) Genetics and Mental Deficiency.
9) Genetics and Neurology.
10) Genetics and Ophthalmology.
11) Genetics and Otolaryngology and Defects of Speech.
12) Genetics and Odontology.
13) Genetics and Orthopaedy, Surgery and Gynaecology.
14) Genetics and Cancer.
15) Genetics and Dermatology.
16) Genetics and Internal Medicine.
17) Genetics and Social Medicine.

Languages of the Congress

The official languages of the Congress will be English, French, and German. The choice of English is, however, recommended for the presentation of papers. The programmes of the Congress will be published in English only.

Proffered Papers

Each full member is entitled to proffer one or more papers which must represent new contributions not published previously. The time allotted for each paper is a *maximum of 15 minutes.*

Application to read a paper should be made on Form B which should be forwarded to the Congress Office together with three typed abstracts in English, French or German (English is recommended) NOT LATER THAN APRIL 1st, 1956. The abstracts will be published when the Congress opens and must not exceed 200 words inclusive of *title, author's name and institutional connection.* Figures and structural formulae will not be accepted.

Authors are asked to specify in which section they wish to present their papers, but the Secretariate reserves the right to make alterations should this be considered desirable.

Papers may be illustrated by latern slides (5×5 cm, 8×8 cm, or 8.5×10 cm) or by films. Details should be given on Form 8. Episcopes will not be provided as it is considered that projection by this means is inadequate. X-ray films to accompany the papers must be copied on lantern slides.

After April 1st, 1956, no applications to read papers can be accepted, as it will be impossible to publish titles or abstracts received after this date in the official publications of the Congress and therefore they will not be included in the programme.

Scientific Exhibitions

A scientific exhibition will be staged in the Institute of Medical Anatomy. Applications regarding scientific exhibits should be made on Form C and forwarded to the Congress Office NOT LATER THAN APRIL 1st, 1956, with full information about the subject of the material to be presented and the facilities required. Only new contributions, not previously shown, will be accepted.

The wall space available for most of the stands is as follows: Length: 100 cm, Height: 122 cm.

Members can apply for more than one stand (separate or adjoining), but as the space is limited the Congress can not guarantee each exhibitor all the space for which he applies.

Place of Meetings

Institute of Medical Anatomy, Nørre Allé 63, Copenhagen N.

Congress Dates

SCIENTIFIC MEETINGS: From Wednesday, August 1st, to Monday, August 6th, 1956.

ENROLMENT AT NORMAL FEE: Up to April 1st, 1956.

Please note: As the hotel situation in Copenhagen is very difficult, members who want the Congress Office to assist them in procuring accommodations must, however, return Form D to the Congress NOT LATER THAN FEBRUARY 1st, 1956. After this date the Congress Office can not take any responsibility for accommodations.

ENROLMENT AT LATE FEE: After April 1st, 1956.

Enrolment

For enrolment of full members as well as associates please use Form A. It would be highly appreciated, if those who are able to make their arrangements well in advance would enroll as soon as possible, as this will facilitate the work.

Up to April 1st, 1956, members will be enrolled at the normal fee. After this date enrolments can *only* be accepted at the late fee.

Categories of Members

FULL MEMBERS: All scientists and members of the medical profession interested in the subjects of human and medical genetics are entitled to become members of the Congress.

Privileges: Admission to all scientific meetings and exhibitions. Issue of all Congress publications. Invitation to social functions.

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FEE: Up to April 1st, 1956: Danish kroner 50.–. After April 1st, 1956: Danish kroner 100.–.

Payment of Fee

Fees should be sent directly to the Congress Office. Cheques must be made payable to THE FIRST INTERNATIONAL CONGRESS OF HUMAN GENETICS, 1956. Only Danish kroner will be accepted. The bank of the Congress is Kjøbenhavns Handelsbank (Branch: 49, St. Kongensgade). The Bank will establish an Exchange Office in connection with the Congress.

NOTE: The date of receipt of the fee will normally be considered the date of enrolment. If the Congress Office has been informed of currency difficulties by the national committee concerned, enrolments will be accepted without the fee, provided that it is stated how the fee will eventually be transferred.

CANCELLATION: If cancellations are received by the Congress Office before July 1st, 1956, the fee will be refunded less 20%. No repayments can be made for enrolments cancelled later than July 1st or to members who fail to attend without advising the Congress Office.

MEMBERSHIP CARDS: When the Congress Office has received tht enrolment forms and fees, the members will be sent a membership card to be presented in the Registration Bureau upon arrival.

Accommodations

DANTOURIST, Copenhagen, has been appointed the official Travel Agency of the Congress and has taken over the responsibility for the accommodations in cooperation with the Congress Office.

Members who want the Congress to procure accommodation for them must return Form D to the Congress Office *not later than February 1st, 1956.*

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POPULATION STUDIES
A JOURNAL OF DEMOGRAPHY

Edited by
D. V. GLASS and E. GREBENIK

Volume IX, No. 2 — November, 1955

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